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THE

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GENERAL SECTION

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VOLUME XX JULY, 1929 PART 1

THE PSYCHOLOGICAL FACTOR IN COLOUR CONTRAST

By JOHN TENNANT

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1. INTRODUCTION.

THE recent work of Prof. Frank Allen of Manitoba is the occasion of and was the incentive to the following article. It has given me an opportunity both of criticizing some parts of Prof. Allen's work, more especially the article on "Reflex Visual Sensations and Colour Contrast," published in November 1923, and of reviewing the subject of Colour Contrast or Simultaneous Induction in some, at least, of its aspects.

I hope in so doing I shall not be thought to underrate the value of Prof. Allen's remarkable contributions to the subject. He has made new discoveries as well as shed a decisive light on old ones. He has produced strong evidence for the fundamental correctness of the Young-Helmholtz theory of vision, he has shown that the third fundamental sensation is almost certainly violet and not blue. He has shown the varying effect upon observations made by one eye, of light or dark sensations in the other, and thus throws some doubt on the accuracy for ordinary daylight of many older observations made in the dark room. Above all, his application of the Flicker method to Auditory, Gustatory and Tactile Sensations has shown that 'reflex effects,' or, to use the old term, 'simultaneous induction,' is not confined to visual sensations, but, at any rate in cases which can be covered by the term fatigue, is a general property of nerve tissue, and thus probably compatible with any theory of vision which suits the other visual phenomena.

His paper on Colour Contrast does not appear to me a very decisive contribution to the special difficulties of a subject in which experiments using fatigue or long fixation can throw comparatively little light.

2. HELMHOLTZ ON COLOUR CONTRAST.

The controversy may perhaps be said to have begun with the publication of Helmholtz's explanation, and is not yet quite at an end, for Dr Edridge Green[1] accounts by a similar explanation for the well known coloured shadow experiment, and von Kries, without absolutely rejecting the direct physiological explanation of Hering, adopts in the main a modified form of Helmholtz's theory.

Helmholtz's explanation was a purely psychological one. In introducing his remarks he used the following illustration: "By the side of a big fellow a man of medium size looks small because we see clearly that there are larger men, but not that there are also smaller ones[2]." The illustration never appeared to me convincing. I pointed out in a paper read before the British Association in 1886 that in the illustration there is an obvious judgment from one thing seen to a different thing seen, from apparent size determined by the relation of two visual angles to real size as measured. But in comparing colours we are dealing with an immediate deliverance of sense, and there is at first sight no reason why we should suppose such deliverance to depend upon a judgment in the way indicated. However[3], the psychological theory of simultaneous contrast cannot be dismissed off-hand, particularly since Helmholtz supported his opinion by many experiments. It is in these experiments that the strength of the theory lies, but I reserve my criticism of them until the evidence for a physiological explanation has been examined and criticized.

---

[1] Edridge Green, *Physiology of Vision*, p. 232.

[2] Helmholtz, *Phys. Optics*, Centenary ed. II, 270.

[3] *Nature*, XXI, 144.

The main arguments for a physiological explanation fall into two classes:

1. Arguments derived from the study of after images, or at any rate from phenomena occurring after considerable fixation.
2. Arguments from the measurability of contrast phenomena.

3. HERING'S CRITICISMS.

Hering, the most important critic of Helmholtz's theory, explored both these lines of argument, but historically his first attack on Helmholtz's explanation was a tract, *Zur Lehre vom Lichtsinne*, republishing a paper read before the Imperial Academy of Science of Vienna in 1873[1].

In this tract Hering describes (*inter alia*) the result of the fixation, say for 60 seconds, of a white square upon a dark ground. The after image is dark on a lighter ground, but is surrounded by a bright halo, of a brilliance so great, that it makes a psychological explanation of the whole appearance quite impossible[2]. He adds to this many ingeniously devised experiments of the same nature which rebut all the endeavours of his opponents, and especially the use made by Helmholtz of the *Eigenlicht*, the unstimulated activity of the retina for the purpose.

It is impossible to read this paper, carefully repeating the experiments, and not to be convinced that the phenomena therein described must receive a physiological explanation[3]. Whether this is decisive of the whole controversy, or whether any experiments under the first head of argument can be so, is another matter.

Hering's experiments were all made in daylight. They are all easy to repeat and are explicable by his theory. To explain them on purely psychological grounds seems impossible. In this particular tract the contrasts are all between black and white, or between darker and lighter surfaces. Prof. von Kries, who may be considered in general a supporter of Helmholtz[4], seems to yield the point in the case of black and white, but to doubt whether contrast of colours can ever be explained on purely physiological grounds.

---

[1] *Lehre vom Lichtsinne*, Wien, 1878, pp. 5–7.

[2] *Ibid.* p. 14.

[3] See McDougall, *Mind*, x, N.S. No. 37, 52.

[4] Von Kries, *Allgemeine Sinnesphysiologie*, Ch. IX.

4. McDougall's experiments.

The next papers I shall refer to are a series of articles published in *Mind* in 1901[1].

These important and, as Sir J. H. Parsons says, these much neglected articles, were an endeavour founded on a brilliant series of experiments on 'fatigue' and after images, to reconcile the Young-Helmholtz theory of vision with the phenomena of colour contrast and successive induction, and to show the inadequacy of Hering's theory to account for all the facts.

A good account of these papers, and of the physiological hypothesis advanced by McDougall, is to be found in Parsons' *Colour Vision*[2].

In these papers McDougall, using a much wider range of observation than Hering, showed that Hering's theory was especially inadequate to explain the positive after images which do not occur in daylight with the moderate illumination used by Hering. He proved that in some cases, a light falling on one part of the retina can inhibit response in another part of the retina which has been weakened by 'fatigue.' He explained the going and coming of the separate parts of the after image by this principle of inhibition, and he compared these phenomena with the well-known case of the competition of different images on the two retinas.

As a result of these experiments he adopted a theory originally suggested by Rollet to account for immediate colour contrast.

Accepting the hypothesis of three independent colours, red, green and blue, he showed that as the grey of the contrasting ground contains all these colours, the juxtaposition, say of a bright red, might well inhibit the red factor in the adjoining grey and so produce upon the grey a blue-green shade.

Among McDougall's experiments supporting this, one may specially be mentioned[3]. In the dark room, a white sheet was placed dimly lit by diffused light. At a distance of 30 cm. on the wall of the dark room facing the light was a circle 5 cm. in diameter which, by means of a photographic shutter, could be suddenly illuminated with bright red light. From a distance of 70 cm., a point midway between the circle and the sheet was fixated for *not less* than 15 seconds, and the bright red circle was suddenly lit up. Immediately the white square became a blue-green of good saturation somewhat varying in tint. The square remained blue-green so long as the red circle was visible, but when the shutter was closed it lost all colour and became slightly tinged with red.

---

[1] *Mind*, x, N.S. No. 37, 52; No. 38, 210; and No. 39, 347.

[2] Parsons, *Colour Vision*, 1st ed. pp. 104–282.

[3] *Op. cit.* No. 39, Observation 38, p. 350.

Prof. McDougall seems to have established as a fact the action of inhibition in this and similar cases. It is true that 'fatigue' takes place in all these experiments, but Rollet's theory seems to give a reasonable explanation of colour contrast, once the natural inference from comparative experiments is accepted, and to remove the reproach under which Young's theory has suffered of failing to account for the phenomena.

The paper deals at length with all the appearances that follow long fixation, and, adopting a chemical theory of the outflow of red-exciting, green-exciting, and blue-exciting substances from a stimulated part of the retina under steady fixation into the neighbouring portions, gives a reasonable account of the origin of the appearances.

Any one who has fixated for some time a red disc, say on a medium grey ground, and has noticed the gradual diffusion of the red colour over the ground, will feel that McDougall's primary hypothesis is a very natural one.

It is, however, possible to admit a physiological explanation when 'fatigue' occurs, and to deny it in the typical experiments from which the controversy began, though Helmholtz did not take that line of reply to Hering's paper.

5. ALLEN'S EXPERIMENTS.

I now turn to Prof. Allen's experiments. A full and appreciative account of these is given in Sir J. H. Parsons' *Introduction to the Theory of Perception*[1].

At the moment I am particularly concerned with two papers published in the *Journal of the Optical Society of America* on the "Reflex origin of colour contrast"; the first and most important in Vol. VII, No. 11 and the other in Vol. IX, No. 4.

The actual method employed in the first of these papers illustrates the general nature of the work, and was as follows:

Prof. Allen[2] first obtained a normal persistency curve by the Flicker method, using a narrow rectangular strip of an arc spectrum seen by the left half of the right eye, and fixating a particle on the edge of the shutter covering the other half of the eye, on the right side of a patch of colour. This normal persistency curve, which is the inverse of the luminosity curve, was thus obtained by slightly indirect vision, but agreed perfectly with the similar curves obtained by direct vision in earlier experiments. The shutter was then transferred to the left half of the eye, and the right half of the eye was fatigued by fixating a patch from a second arc spectrum, again focusing upon a particle on the edge of the dividing shutter. The effects of this fatigue on the normal curve could thus be investigated. I gather from a former paper[1] which traced the effects of fatigue of one eye on the other eye, that the arc spectrum was so bright that the colours in the middle part of the spectrum "approached whiteness in appearance[2]," and that fixation was continued for two or three minutes. By the method used, 'fatigue' could be confined to one retinal area, and the induced effects, if any, transferred to an adjoining area could be measured.

---

[1] Parsons, *Introduction to the Theory of Perception*, Ch. XII, p. 196.

[2] *Op. cit.* VII, 915.

Prof. Allen's results are summarized in Sir J. H. Parsons' treatise[3] before mentioned in the following manner:

1. Stimulation of one area of retina with light causes:
   - (*a*) Direct effects upon that area.
   - (*b*) Induced effects in all other areas, *i.e.* both in the same eye and the other eye.
2. Direct effects—with both eyes in a condition of moderate photopia:
   - (α) Stimulation with white light depresses the activity throughout the spectrum.
   - (β) Shading the eye also depresses the activity throughout the spectrum.
   - (γ) Stimulation with any given wave-length depresses the sensitivity to this region of the spectrum and enhances it for the complementary colour.
   - (δ) With any given wave-length the depressions and enhancements show three curves affecting respectively the red, green and violet.
   - (ϵ) Stimulation with the equilibrium colours $660\,\mu\mu$, $570\,\mu\mu$, $520\,\mu\mu$, $505\,\mu\mu$, $480\,\mu\mu$, $425\,\mu\mu$ has no apparent effect upon the sensitivity or the visual apparatus.

---

[1] *J.O.S.A.* VII, No. 8, 585.

[2] *Ibid.*

[3] Parsons, *op. cit.* Ch. XII, p. 207.

3. Induced effects: With both eyes in a condition of moderate photopia, the effects on the unexcited areas of the same and the opposite retina are:

(α) Stimulation by white light enhances the sensitivity throughout the spectrum.
(β) Shading an area or an eye depresses the activity throughout the spectrum.
(γ) Stimulation with any given wave-length enhances the sensitivity throughout the spectrum, the enhancement being most marked in every case in the red, green and violet.
(δ) Stimulation with any given wave-length enhances the sensitivity most for the complementary colour.
(ε) Stimulation with the equilibrium colours has no apparent effect.

6. CRITICISM OF ALLEN'S RESULTS.

In this paper, I am chiefly concerned with the induced effects under the head 3 (δ), but I make one general criticism.

It appears to me that the brilliance of the illumination used, and the long fixation, introduced complications which render generalizations of a quantitative nature for lower illumination and shorter fixation somewhat uncertain, and may seriously affect the conclusions drawn[1].

A more recent paper, using varied and lower illuminations, has given valuable results concordant on the whole with those already arrived at, but the long fixation, which is inherent in the method, makes it scarcely possible to apply the conclusions derived from these laborious investigations to any visual experiences in which fatigue is not a factor.

An experiment of Prof. McDougall points to the danger of relying for quantitative results on such long fixation as two or three minutes, especially with the brightness used in the middle parts of the spectrum. Prof. McDougall[2] laid a disc of red paper on a grey ground in strong lamplight. After two and a half minutes' steady fixation, the red actually passed through yellow to a bright blue-green, changing back after a few seconds to red and then to green, with several alternations. Evidently such prolonged fixation has very complicated effects, and such complication will probably not be confined to the direct effects, but will affect the induced effects in adjoining areas. Taking this into account and other experiments in these admirable papers in *Mind*, it seems somewhat dangerous to make quantitative inferences from the experiments of Allen to cases of much shorter fixation, and to transfer them to the typical case of immediate and instantaneous colour contrast seems quite inadmissible.

---

[1] *J.O.S.A.* XIII, No. 4.

[2] *Mind*, X, N.S. No. 38, 221.

To turn to the specific point of simultaneous contrast. What is the exact type of experience which Prof. Allen has analysed by the spectrometer?

The following is a fairly close parallel. Take a square of ruby glass and divide it into two by a black card, point to a white cloud and fixate the middle point of the card-edge with the right eye for two or three minutes. Then cover the ruby part of the glass and notice the after image, say, on a grey ground.

Now in such an experiment the complementary colour, if seen at all, is seen only in the first few seconds. If I use a small square patch of grey paper and illuminate it from behind, I do see it at first, but it rapidly vanishes and in both cases the red colour begins to invade the dark part, and a rosy flush, deepening as fixation continues, covers the neutral part of the glass.

It would seem, from these experiences, that we should expect the induced effects in Allen's paper to be similar and not complementary to the fatiguing colour. When we look carefully at the figures in Prof. Allen's paper, it appears that the conclusion 3 (δ) is not legitimately drawn from them. In some of the figures it is the more probable inference from the facts, but three of the figures show not the reinforcement of the complementary but of the inducing colours in the other half of the eye.

Very clear is this in the case of ·588 and of ·410 and fairly evident in the case of ·676[1]. It does not appear to me that these results of long fixation throw any but indirect light upon the main subject of the present paper.

The conclusion I arrive at is that no demonstration that colour contrast has a physiological origin can be derived from experiments of the 'fatigue' type, but Rollet's theory with McDougall's experiments gives a persuasive explanation of the result, and Prof. Allen's demonstration in his long series of experiments on the various senses that induced effects are a general property of nerve tissue, throws the burden of proof upon any who still uphold a purely psychological explanation of the induced phenomena.

---

[1] See *J.O.S.A.* VII, 919, 926, 917. See also reflex curves in Parsons' *Introduction to the Theory of Perception*, p. 201.

7. FURTHER EXPERIMENTS ON THE OBJECTIVE CHARACTER OF COLOUR CONTRAST.

The second head of proof that the simultaneous contrast effects are objective is derived, to put it broadly, from the fact that they are measurable[1]. Hering and others have made many measures chiefly by means of the colour top and their conclusions are summarized by Parsons[2].

I have myself made similar experiments with a clockwork colour top having two discs side by side. The method I used was to arrange a grey strip on one disc on a black ground and a grey strip of like tint and breadth on a coloured ground, and then see either how much of the ground colour must be added to this disc to match the first, or what colour has to be added to the first to match the latter. I have not made a long series of systematic experiments, but in a general way I have verified Hering's conclusions, especially that the more saturated the coloured disc the stronger is the induced effect.

The objection which can be made to these experiments depends on the difficulty of excluding successive contrast. It must be admitted that the danger is real. If I place a narrow strip of saturated colour on a grey ground, a single second's glance, followed by a rapid removal of the coloured strip, shows a complementary coloured strip which fades with great rapidity. But in the series of experiments, by Hering, Hess, Pretori and others, great pains were taken to exclude successive contrast, and the results seem to be reliable[3].

It is not, however, necessary to measure the effects of contrast in order to demonstrate the physiological explanation. It is sufficient to show that in the same field of vision in close proximity, and if necessary with strict fixation, two grey strips of the same colour appear different with different surroundings.

A single page of pictures in Hering's *Lehre vom Lichtsinne*, Tafel II, opposite p. 116, should suffice to carry conviction as regards the contrast of black and white.

The following modification of the experiment on p. 117 of that work is to me extraordinarily convincing and easy to verify.

Punch a hole in a fairly thick white card, and on a table facing a window, lit by diffused daylight, place a similar piece of white card slightly inclined away from the light. Then, holding the punched card parallel to the window, in such a position that a small portion of the fixed card is seen through the hole, slowly revolve it from a position fully facing the light to a position quite turned away from it. The changes from dark to light of the visible portion of the fixed card are astonishing. It is instructive to vary the operation by keeping the upper card fixed and revolving the lower. In the former case the light which the fixed card sends to the eye is the same throughout, and the apparent changes due wholly to induced effects; in the latter case the light itself varies, and yet the changes, though their range is somewhat greater, are hardly more remarkable.

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[1] Parsons, *Colour Vision*, 1st ed. pp. 107, 128.

[2] *Ibid.* Sect. III, pp. 263, 266.

[3] For another method of measurement not dependent on rotation, see Hering, *Grundzüge der Lehre vom Lichtsinne*, p. 126.

Now, using a smaller hole, say a quarter of an inch in diameter, take two white squares and make the holes in each an inch from the side, so that when the squares are placed side by side the holes are two inches apart and easily fixed in the same field of view. Then, in repeating the former experiment, revolve the upper squares in opposite directions. There is seen in the same field of view two circular patches, one nearly black, the other nearly white, and yet both sending the same objective light to the eye. There is here no question of merely judging a relation, but a direct comparison of the patches themselves, and the conclusion is surely irresistible.

The colour experiment I suggest is, I believe, new, and as it makes use of coloured shadows is specially interesting.

I filled two electric torches with Wratten's gelatine discs. One was covered with a red disc (Wratten's filter A.) which excludes all the spectrum of shorter wave-length than $600 \mu\mu$. The other was charged with a green.

In a room lit by not very strong diffused daylight, and on a white ground, I placed a cylinder, say two-thirds of an inch in diameter. The electric torches, in order to narrow the beam of light, were placed in tubes with blackened insides and with the bulb about one inch from the end. They were then held in a position at right angles to one another and directed at the cylinder. Then the light was turned on. The shadow of the red torch was blue-green, and of the green violet. Both were in the field of vision at once, both sent the same light to the eye, viz. the dim daylight of the room, and yet they looked absolutely different. Moreover, this is so for a momentary exposure.

These experiments demonstrate the reality of a physiological factor in simultaneous colour contrast. It probably depends, as McDougall has explained, on the principle of inhibition. It is, however, possible that reflex effects of a positive nature, such as Allen has shown to exist in cases of long fixation, may also affect the results.

8. PSYCHOLOGICAL FACTORS IN COLOUR CONTRAST.

The question then arises, how are we to explain the adherence of a brilliant intellect, such as Helmholtz's and of many lesser men, to the purely psychological explanation? Why, for instance, in the ordinary course of things do not people notice the contrast effects more commonly, and why does it need special arrangements, coloured glasses, coloured shadows, spinning tops, tissue paper covers, to reveal them?

There are, I think, two factors of a psychological character which explain this.

The first was pointed out in a paper I read before the British Association in 1886. We are not in practical life concerned with the varying deliverances of sense, except so far as they interpret for us the outside world which vitally concerns us. What matters to us is not the play of light and shadow but the local colour, the appearances which betray and reveal what the object is[1]. For the lower animals it must have been a vital necessity to recognize local colour so as to identify their foes or their prey, and at any rate in the long course of human evolution this has been true. We have been trained as a race by rapid inference under varying circumstances to recognize the normal colour of things seen[2], and quite unconsciously our judgments concern ourselves with that.

Helmholtz has stated this law of vision as follows: "In our observations with the sense of vision we always start by forming a judgment about the colours of bodies by eliminating the difference of illuminations by which a body is revealed to us."

Anyone who possesses a series of grey papers of various tone has at hand a simple and striking means of illustrating this law of vision. A subdued grey, close to the single window of a room, may send more light to the eye than a white paper at a distance from it, and yet the one will appear white and the other grey; and the light greys can be picked out from the dark, irrespective of whether, in their particular relation to the light, the dark one sends more actual light to the eye than the lighter one. The true luminosity relations can be ascertained either by isolating each paper from its surroundings by the use of a tube, or, if you can get them side by side in the same line of vision, by closing one eye and confining the attention to the surfaces compared.

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[1] Von Kries, *Allgemeine Sinnesphysiologie*, Ch. IX, p. 260.

[2] Hering has called this "die Konstanz der Sehdinge." I shall refer to it as the principle of the recognition of normal colour, see von Kries, *Allgemeine Sinnesphysiologie*, p. 260.

Hering has shown, by an experiment in which a white wall opposite the single window of a room appears darker than a grey paper near the window, that this recognition of normal colour takes place under the simplest conceivable circumstance, and if Helmholtz differs at all, it is only in making our judgment more deductive and less instinctive than it really is.

In the experiment with the greys we actually see, not what pure untutored vision would affirm, but what the totality of our living experience informs us. Thus the psychological factor is real, but usually works, not as Helmholtz believed, to make us see colours which have no physiological or objective foundation, but to make us ignore colours which are not permanent property of the objects around us.

The second factor of a psychological nature that affects our perception of tint is this. The very fact that we recognize a grey, a white, a coloured object under various illuminations, means that our standard of colour is not a very nice one. This is especially the case with greys, much less so with saturated colours. I have a series of grey discs, procured many years ago from Hering's makers, some of which seem to me too blue, and others at times too brown. The spectrum under scotopic conditions seems to me pure grey, but others think it has a bluish tinge, nor can I make up my mind as to which of the greys in the French colour chart is pure neutral grey. If one compares a mixture of two complementary colours on the colour top with a mixture of black and white, it is surprising how the delicately tinted colour disc affects our judgment, the one certain judgment being a relative one, that this is on the purple side of grey and that on the green, or *vice versa*. Hence Peddie is quite right in saying that in the case of white or grey we have no very exact standard to guide us[1], but he has hardly indicated the true factor when he adds that we regard the brightest light as white. The brightest light, when the sun looms red through mist, is red, but white paper looks white, and grey paper grey, under such illumination, quite irrespective of the actual physical rays sent to the eye. This result is possibly largely of a physiological character, but I think the top experiences referred to show that there is a psychological factor as well, and that in comparing two greys the relative judgment is much easier and more certain than the absolute one. We can say this grey is on the blue side of that, much more confidently than we can affirm that the one is tinted, the other pure[2].

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[1] Peddie, *Colour Vision*, p. 17. [2] See Helmholtz's *Phys. Optics*, Centenary ed. II, 2.

9. DISCUSSION OF OTHER CLASSICAL EXPERIMENTS.

I shall now consider the classical experiments, and I turn first to the coloured shadow experiment which I shall discuss at some length.

(a) *Helmholtz's coloured shadow experiment.*

This consists essentially in comparing the two shadows cast in a dimly lighted room by diffused daylight, and a yellow illuminant on white paper. The eyes should be previously covered, and one spot fixated to avoid the effect of successive contrast. The shadow cast by daylight and lit by the candle is yellow, as it should be, that cast by daylight is not grey, but blue. A cloudy day should be chosen, as shadows lit by a blue sky are naturally blue.

In making the experiment Helmholtz used a blackened tube, fixated a spot in the candle shadow so that only the candle shadow appeared in the tube, and noted its unaltered character whether the candle be lit or not. He said[1]:

> On the other hand, if the tube is held to the eye and directed so that a part of the field surveyed is illuminated by the red-yellow light of the candle, the shadow from the candle again becomes blue. When the blue has become really intense let the tube again be pointed so that nothing but the subjective blue is in the visual field. The blue now persists whether the candle is allowed to shine on the rest of the paper or whether it is screened, which of course, so far as the observer is concerned, amounts to the same thing because under these conditions he is not aware of it at all. The blue colour in such case is so constant that Osam has concluded from similar experiments that it is objective. This opinion is easily refuted by the fact that the blue colour persists even when the candle is extinguished. But at the moment when the black tube is removed from the eye the subjective blue disappears also because it is then recognised to be identical with the white that occupies the rest of the visual field. No experiment shows more impressively or more clearly the influence of judgment on our determination of colour. As the result of the contrast whether it be successive or simultaneous, once the judgment has been formed that the colour in the shadow of the candle light is blue, the colour continues to appear blue although the circumstances that led to the decision have ceased to exist; until the black tube is removed so as to enable us to make a new comparison with other colours and to form a different judgment in the light of new facts.

(b) *Hering's modification.*

Hering repeated this experiment[2] of Helmholtz with elaborate precautions. His tube was split in half in such a way that it could be made to fly open by touching a spring. He used a lamp that could be extinguished or turned on at will. He made the following preliminary experiments. He first gazed at the white field with the lamp out; then lighting the lamp, he noted the yellow tint added to the field which gradually faded under fixation. Putting out the lamp, a blue after image appeared, whose intensity depended on the length of the former fixation but was visible even after a second and did not depend on the use of the tube. He then, using the tube with lamp out, fixated the white ground for 20, 40, or 60 seconds, and noted the after image which sometimes showed weak coloration and needed for distinctness careful and steady fixation. Now lighting the lamp, he fixated a portion, and putting out the lamp, noted the blue after image. Then touching the spring, he opened up the tube and the image lost blue, the colour becoming fainter.

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[1] Helmholtz, *Phys. Optics*, Centenary ed II, 272.

[2] *Archiv fur Phys. Pfluge*, XL, 172.

Hering's explanation of this is difficult, but it is easily understood from Prof. Allen's experiment. It turns on the effect of the presence or absence of the tube, and takes an important place in Prof. Allen's criticism of Helmholtz.

After these preliminaries Hering, using the tube, fixated the edge of the candle shadow, and saw an after image half blue and half grey or even yellow on moving the field of view wholly into the candle shadow. Then again putting out the lamp and opening up the tube, he saw an image partly blue and partly grey, thus proving that the prolonged vision of blue was the result of successive contrast.

Hering pointed out that the fact that the blue in Helmholtz's experiment is described as developing strength with time, showed that fixation must have been imperfect, as colour contrast was greatest in the first instant and diminished, and could even be reversed by long and steady fixation.

Allen has also discussed Helmholtz's coloured shadow experiment and agrees with Hering that the continuation of the blue image is due to successive contrast.

(c) *Edridge Green's coloured shadow experiment.*

Edridge Green[1] has, however, devised an experiment with a rather unexpected result which at first sight strongly suggests a psychological explanation of the coloured shadow experiment, and which at any rate demands careful examination.

He says:

> If in repeating the experiment the opaque object be placed upon a dull black surface and two pieces of white paper be placed on this surface for the shadows, care being taken that these pieces of paper are the exact size of, or smaller than the

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[1] Edridge Green, *Physiology of Vision*, p. 232.

shadows, these will appear blue and yellow as before. If we now place a small dot on the paper on which there is a blue shadow, and having covered one eye keep the other rigidly directed at this black spot while an opaque object is placed in front of the candle so that it no longer illuminates the paper or throws a shadow, both pieces of paper will appear white being illuminated only by white daylight. The eye being still rigidly directed on the spot on the paper, the opaque object is removed so that the candle again throws a shadow on the paper. The shadow thrown by daylight immediately appears yellow of greater saturation than before, but the shadow thrown by the candle appears white as before, and without the faintest trace of blue or blue colour. The conditions are everyway favourable to the development of the blue colour but none appears, because the observer has been able to form a correct estimate of white. If the blue colour were a real subjective coloration caused by the yellow, blue should appear on the shadow from the candle.

In repeating this experiment, I made some modifications which gave interesting results.

(*d*) *A new form of the shadow experiment.*

I used, as the black cover, a black flock paper, reflecting the least light possible, and for a white ground a piece of pure white card. The flock was about a yard square and completely covered the table used. In all the experiments the position of the light and the opening of the shutter were so arranged as to give the richest and fullest effects with the white ground. A cylinder caused the shadows, and in all cases it stood on a circular card of black flock which hid the junction of the shadows. I prepared two white papers, as in Green's experiment, marked in the centre with a spot of ink for fixation. Besides these, I prepared two cards covered with black flock and long enough and broad enough to cover the shadows completely.

I made some preliminary experiments, using the white ground. I covered the two shadows with the black cards, and, after resting the eyes and fixating with one eye either the card covering the candle shadow or a point between the two shadows, I sharply removed both cards and noted the colour of the blue shadow. Replacing the black cards and fixating as before, I now removed only the card covering the candle shadow. I could see no difference whatever in the blue colour, nor did it matter to the colour whether the daylight shadow was covered or uncovered. In the classical experiment, then, it is the ground colour that matters, and the presence or absence of the daylight shadow is a matter of indifference. In these experiments the eye should be rested from time to time, and long fixation of the blue shadow which causes fading, avoided.

I now fixated the blue shadow steadily and then cut off the light both from the ground and my eyes. I saw a pale yellow after image of the candle shadow surrounded by a rather rich blue ground. The after image of the daylight shadow was too dark to show definite colour.

I now used the black ground and, with candle lit, placed the white paper on the shadows, covering them at first with the black cards. If I accustomed my eyes to the doubly lit room, and then removed the black cards, the candle shadow appeared a faint blue but nothing like so saturated as the same shadow when the ground was white. If now I rested my eyes and, keeping the light from my eyes, I fixated the candle shadow as in Edridge Green's experiment, the paper looked white, but I could not convince myself that the daylight shadow appeared more saturated. If I again shaded the light and now fixated not the white paper but a black flock card covering it, and then sharply removed the black cards from both shadows, the candle shadow was white as before, though in this case I had not accustomed myself to the white of the cover. Moreover, when, after letting my eyes wander, the candle shadow appeared blue, it hardly seemed to alter at all whether the daylight shadow was or was not covered by the black card, again pointing to the fact that some other factor than the yellow shadow was at work in causing the blue colour.

On the other hand, if I used instead of the white cover a quite *small* rectangular white stripe, and placed it across the candle shadow so that one part projected beyond the edge of the shadow, then, though I covered the daylight shadow with the black card, and with candle lit fixated the black card over the candle shadow and then sharply removed it, the blue of the shadow was present in spite of the small size of the lighted ground near it.

These experiments with a candle were made in a room rather darkened by plane trees, and the blue of the shadow when the black ground was used was always faint.

I made similar experiments in a room well lighted, on a cloudy day, using an electric lamp with a white shade which could be turned on or off at pleasure. In this case the blue tint of the candle shadow when the black ground was used was more evident than in the candle series, but it still bore no comparison to the colour of the same shadow when the white ground was used. It was also in this experiment much more difficult, by fixating the candle shadow with lamp out and then getting the light turned up, to get rid of the blue, and indeed in the small rectangular stripe modification it was impossible if I was facing the light. In all cases the rectangular stripe within the shadow appeared blue and the projecting part white.

If, however, I did not face the light but stood by it, carefully shading my eyes when I turned it up, and then, with the light out, fixated the transverse rectangular stripe and turned on the light, I then saw the part within the shadow white and the projecting piece lit by the double light of lamp and daylight a good yellow. The daylight shadow was covered by the black card in both these experiments.

(e) *Discussion of results.*

It seems clear from these facts that some other factor than pure physiological contrast is operative, and clear also that it is the doubly lit ground that matters and not the other shadow. I say another factor because the effect on the production of blue of so very small a portion of the doubly lit ground as the stripe is undoubted, and in the last experiment made was shown not to operate constantly but to depend on suggestion.

Edridge Green's explanation might possibly be good for very faint colours, slightly differing from white, and might possibly apply to the last experiment recorded as between the two whites of the singly lit and doubly lit ground, but, used as a general explanation of the classic experiments with shadows or similar strong effects, it fails to account for the fact that contrast colours are always complementary, or nearly so, to the surrounding ground. Not only has Rood shown that our memory for full colours is good, but if we are dependent on estimates of the ground colour there seems no reason why the contrasting effect should not sometimes resemble the ground. Take the case of a grey strip on a red ground. If we correctly estimate the saturation and tint of the ground the strip should look grey, if we overestimate its saturation and colour the strip should look blue-green, if we underestimate its saturation the grey should, according to Edridge Green, look reddish. I cannot think that estimates by memory play any considerable part in the phenomena, and the above experiments show that the daylight shadow, at any rate, is not an operative factor.

In all these experiments, with a room partly lit by daylight and partly by yellow light, except with very special precautions such as those used in the final experiment, we actually see the ground white. It seems to be a direct perception and not an estimate, and seems to point to that adaptation of the eye to changes in general lighting which I have already insisted is a consequence of the necessity men and animals are under of recognizing colours as objectively the same under varying conditions of illumination.

Is it then a physiological or a psychological effect? Are the sensory impulses so directly modified by the general lighting, that they produce the same effect as under fatigue; or are we to explain the result not in terms of sensation but of direct perception as, without doubt, the recognition of the darkness of the different greys is to be explained in Hering's experiment? It is not, it seems to me, psychological in the sense that we consciously compare the white of the ground with our remembrance of a standard white, but it is a perceptual reaction to the whole surroundings in which we are placed, and this reaction is explained and necessitated by the evolution of mankind which has made the recognition of the normal colour of objects a vital necessity. This does not disprove the simultaneous action of the physiological factor, which, in the very case of coloured shadows, the experiment with two electric torches proves to be operative.

The results of the last experiment render me doubtful if Hering's explanation of Helmholtz's results is completely satisfactory. It seems at least possible that the suggestion that the shadow is blue continues to operate when the light is put out, and that the sudden change to white is explicable, as von Kries[1] holds, by those effects on our perception which he calls 'Umschaltungen.' For these, and for the general treatment of psychological effects on perception, I must refer to his valuable chapter. My chief difference from him is that, while he admits the simple physiological factor in the case of black and white, he doubts its operation with colour. This error, as I regard it, seems to me adversely to affect his judgment in several places.

(*f*) *Ragona Scina experiment.*

I pass on to discuss at some length another of the classical experiments, that of Ragona Scina, as I think it throws, when carefully considered and varied, a valuable light on the whole controversy. It consists essentially of two square boards at right angles, which I shall call the floor and the wall. Both are covered with white paper, and a plate of coloured glass divides equally the angle between them. The wall with the glass and floor in front of it is placed facing a window. On the floor is a pattern made in black and white, and on the wall the same pattern with the black and white interchanged. The patterns are so adjusted that the reflection in the glass of the wall pattern coincides with the pattern on the floor transmitted through it. The white light reflected from the glass when the glass is not too thin or transparent causes a grey pattern to replace the black of the floor, while the white parts of the pattern take a deep colour from passing through the glass. The general white of the floor when opposed by the white of the wall causes the colour of the ground to be diluted with white.

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[1] Von Kries, *op. cit.* For the shadows see p. 276.

The grey parts seem tinged with a colour complementary to that of the glass. Thus when the glass is green, the grey looks a pale rose or violet, and when the glass is blue, yellowish. If the experiment is made without due care, by allowing the gaze to wander over the pattern, the contrast colours are fairly vivid, though never, I think, quite so unmistakable as the coloured shadows in the last experiment, but in this case successive contrast is operative, and great care is needed to make the experiment accurately. It is important to adopt Hering's method, and to interpose in front of the pattern a black screen. This can be fixated, removed, and replaced after a few seconds. When this is done, the complementary colour is much less vivid, successive contrast having been excluded.

(g) *Helmholtz's modifications.*

Helmholtz notes that there must not be too much difference in luminosity between the grey pattern and the ground[1], and he recommends that when the coloured glass transmits so much light as to show the dark parts of the pattern too dark, it should be covered with white tissue paper. I have found that in such cases it is effective to cover the coloured glass with a colourless one, thus increasing the number of surfaces that transmit white light to the eye. Helmholtz, after describing the arrangements necessary for the experiment, continues thus:

> Now if the observer will find a grey paper of exactly the same colour as that in which the spot 'e' (*i.e.* the dark part of the floor pattern) would appear to him without contrast, and hold a small piece of it over the coloured plate so as to hide half of the spot 'e', this little piece of paper will not appear in the complementary colour at all or even show any suspicion of it; and the moment the colour of the spot 'e' is compared with it and seen to be the same, the complementary colour of 'e' disappears also and changes into a simple grey.

(h) *Hering's criticism.*

Hering[2] has published a careful criticism of this experiment. He used, instead of the small square patch 'e' used by Helmholtz, a pattern of black and white concentric rings. He enclosed the whole apparatus in a dark-lined box, with one side open to the light, using the side facing the wall. He covered this with a lid in which was a circular hole, through which the pattern was seen and which completely hid the edges of the plate. This aperture was covered, a spot of the pattern fixed, and the cover removed. Even children saw the contrast colour. Then, like

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[1] Helmholtz, *Phys. Optics*, Centenary ed. II, 283.

[2] *Archiv für Phys. Pfluge*, XLI.

Helmholtz, Hering brought a selected grey disc above the glass matching the grey of the pattern. He agrees with Helmholtz that both look alike, but affirms that the grey disc as well as the pattern show the complementary colour of the ground.

I do not need Hering's elaborate arrangements to agree with this decision. I see both the grey paper and the pattern reflected through the glass alike, showing the complementary colour of the ground.

It is to be noted that Helmholtz himself, in the passage I have previously quoted, admits and indeed insists that there is a tendency to see objects in their normal colour in spite of strange lighting and other illuminative effects, and Hering's arrangements, by excluding the presence of other subjects, may be adapted to counteract this tendency, and so make it easier to recognize contrast effects, if such exist.

Thus the difference between the two is this:

Both agree that the grey paper matches the pattern, but while Hering insists that the contrast colour is a real effect due to a physiological cause independent of any act of judgment conscious or unconscious, and would, I suppose, explain Helmholtz's failure to recognize it, by the tendency which Helmholtz himself admits in some cases to ignore colour resulting from position and special circumstances, and to see only the normal colour of the object, Helmholtz accepts the pure grey of the paper, as given directly in sensation, and gives an explanation of the colour seen through the glass which appears to me more ingenious than convincing. He says:

> The observer[1] decides that the black spot on the horizontal surface is pink-red, but he also decides that this spot as well as the entire surface with its pink-red colour is seen through the green glass and that the green colour given by the glass extends uninterruptedly over the entire lower surface and even over the dark spot. Thus he believes that he sees two colours together at this place, that is, green which he attributes to the glass plate, and pink-red which he attributes to the paper behind it and the two of them together do give the true colour of this place, that is white. As a matter of fact an object which seen through a glass sends white light to the eye as this spot does, would have to be pink-red. But when a white object of exactly the same appearance is placed above the plate of glass every reason for resolving the colour of the object into two disappears and it looks white to us.

This appears to assume that a colour passing through a coloured glass is added to the colour of the glass, whereas of course the result is incalculable beforehand and depends on the particular constitution of the glass. Moreover, as Hering has remarked, children see the complementary colour. Are they aware that rose and green are complementary, and do they indulge in these elaborate calculations? Supposing in Helmholtz's argument we substitute a blue glass for the green one. The yellow of the pattern is quite as vivid as the rose in the case of green, but far from thinking that yellow and blue make a white or grey, ninety per cent. of people even to-day think they make green, and yellow seen through most blue glasses does actually look green. To estimate the black parts of the pattern as yellow in the way Helmholtz suggests, the light sent to the eye from them should be green and not grey as, according to Helmholtz, it actually is.

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[1] *Op. cit.* p. 286.

Hering's interpretation is strongly borne out when the experiment is made a comparative one.

(i) *New form of the Scina experiment.*

To this end I used, instead of a single coloured glass for the whole field, two strongly contrasting glasses, one a full rich blue, and the other a fairly deep yellow. They divided the field centrally into a right and left half, so that one half was seen through the blue glass and one half through the yellow.

It was easy to verify by covering the whole floor with a black flock paper, that the light actually sent to the eye by the dark parts of the pattern was tinged by reflection by the colour of the overlying glass.

I used for a pattern in the first experiment, a double **V** like the pattern used by Hering to illustrate the contrast of black and white. The double **V**'s in the pattern on the floor were black on a white ground, and so arranged that their apices met on the central line just under the division between the glasses.

If the experiment was then made with perfect strictness, the difference between the **V**'s was manifest, the one under the yellow looking bluish, and that under the blue, yellow. All inexperienced people saw this difference, though some might call one arm grey and the other coloured.

It is a fact, and one that goes far to refute Helmholtz's explanation of the Ragona Scina experiment, that the contrast between the two **V**'s can be just as easily seen without the elaborate arrangements of the experiment.

Over one half of a rectangular grey paper I pasted a saturated blue paper, and over the other a saturated yellow paper, leaving a double **V** of the same grey as the ground to meet at the apices on the line separating the blue and yellow halves. Using similar care to avoid successive contrast, the two greys were just as sharply contrasted as in the former experiment, and observers were astonished when they were told that they were both exactly the same grey colour, apart from their surround-

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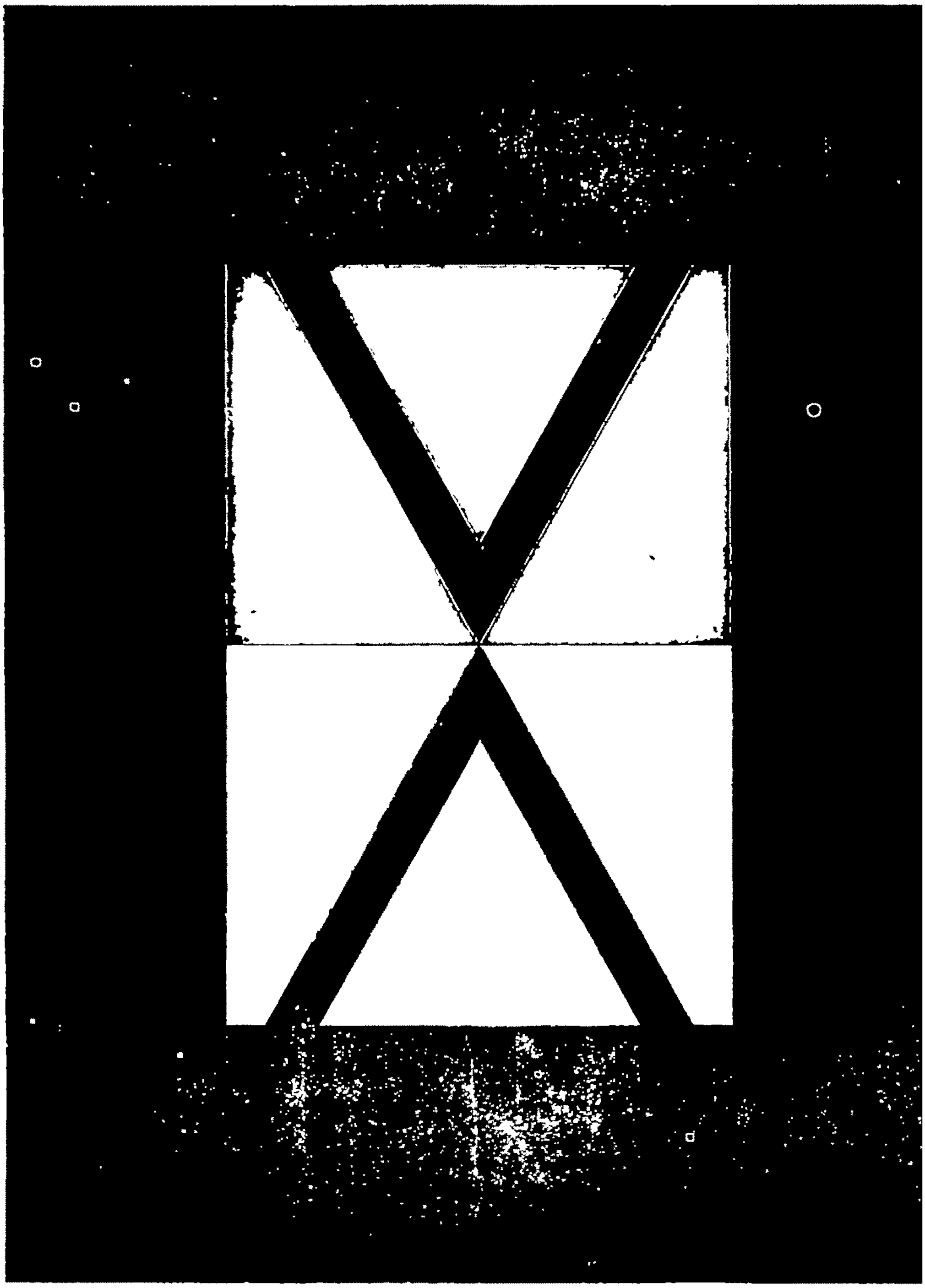
ings. I think it is very significant that when a comparative method is used, the artifice of the Ragona Scina experiment has no advantage in the display of contrast over the most straightforward proceeding. Recurring to that experiment, I tried the V's in various positions, also the target type of pattern used by Hering, but when proper care to avoid successive contrast was taken, the contrasting colour always appeared under the respective glasses. This was also the case even when a straight bar pattern was used, the contrast in this case being more marked than in the parallel experiment with grey paper. In the latter case, when a bar of suitable grey crosses the centre of a paper of which the right half is green or blue and the second half yellow or red, the contrast effect is absent or faint. There is a distinct tendency to see the bar of a uniform grey, and many people call it so. I myself always seem to see some difference between the two sides, and if a black strip separates the two sides the contrast becomes manifest. I think that the division between the two glasses in the Ragona Scina experiment plays the part of the black strip in the case of the coloured papers.

It appears, then, that for some people at any rate, the continuity of the grey strip suggesting a uniform colour prevents colour contrast being seen even in a comparative experiment. This result is even more marked if the grey strip is part of a larger sheet of which the comparative experiment occupies only a small portion. In this latter case not even the use of tissue paper seems to me to make the contrast colour visible. If, however, the central portion containing the comparative experiment is isolated from the rest by a narrow black strip and a black strip separates the colour, the contrasting colours on the grey bar are manifest.

There is an interesting variant of this experiment upon a general ground of grey paper which both emphasizes the power of suggestion in visual perception, and strengthens the case for simple induced effects in Hering's sense. If I place in the centre of a grey field double contrasting V's occupying rather a small proportion of the whole field, say a sixth or an eighth, so that the grey portion of the double V's forms a part of and is continuous with the field, and then fix the apex of the V's, not only are the contrast colours of grey strips which converge to this apex plainly visible, but the contrasting colour extends to each half of the whole field. By fixating, not the apex of the V's but a point in the grey field just above the vertical line bisecting the V's, I can recover the impression of the uniformity of the field, but I cannot abolish the difference between the two strips which converge to the apex of the V's.



TENNANT.—The Psychological Factor in Colour Contrast.

10. METHODS OF INCREASING SIMULTANEOUS CONTRAST.

Lastly I shall discuss the well known and sometimes rather surprising effects of tissue paper in increasing the perception of contrast.

Prof. Allen's explanation of this seems to me quite inadequate. He says[1]: "The tissue paper reflects upon the active retinal areas a small amount of white light which contains all the colour of the spectrum. An additional stimulus for each enhanced sensation is thereby provided and the reciprocal modification of the colour is made more effective and complete." There are two pertinent criticisms to be made on this. In the first place, as may easily be verified, the tissue paper does not in all cases add white. A pure white disc is darkened by it, the loss by absorption being greater than the light reflected from the surface. If the white content is increased, as is the case with nearly all greys, the increase in white value can be imitated by using a lighter grey on the surface of the tissue paper, or by using a lighter grey without using the tissue paper at all.

In the second place, no account is taken in this explanation of the much diminished saturation of the colour of the inducing field, and the consequent weakening, according to the measurements of Hering and others, of the induced effects[2]. Indeed, with such tissue paper as I have used, the inducing field is sometimes so faint and irregular that some people see the contrast colour on a dark disc placed below it almost more clearly than the colour of the ground itself. In some of these experiments the very differences of different observers seem to suggest a factor other than a purely physiological one.

I find that a thin glass carefully ground may be used to replace fine transparent tissue paper with somewhat similar results, but in this case the greater transparency of the glass diminishes much less the saturation of the ground colour, and makes much less change in the apparent white content of any grey placed below it.

I covered a dark but richly coloured and saturated royal blue paper, with tissue paper of exactly the same size and above that a finely ground thin glass of like size and concealed the edges by a black frame. The blue showed so faintly that some failed to recognize it, but a black flock paper placed below not only made the blue manifest, but showed to most people a yellow tint quite as definite as the blue itself.

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[1] *J.O.S.A.* IX, 383.

[2] See Parsons' *Colour Vision*, 1st ed. pp. 127, 128, 264.

If I placed a light grey on the surface I could see no contrast colour when, as in this case, the ground colour was so faint.

If, instead of royal blue, I used a bright saturated green, the ground colour was more manifest, and to me a grey placed on the surface showed the complementary purple, though not to all observers, and a black disc placed below the tissue not only accented the manifestation of the green, but the contrasting purple was visible to everyone.

If I placed a grey disc above so that it lay partly on the ground glass and partly on the black frame, no colour was visible on the grey. If, instead of using both tissue paper and the ground glass, I used the ground glass only, the contrast colour was manifest to me whether I placed a grey disc below or on the top of the glass, and it was possible to so choose the upper grey as to make a fair match with a very slightly darker grey placed below, thus repeating the experiment which both Helmholtz and Hering made with the Ragona Scina experiment with such diverse conclusions. In this case I always see the contrast colour on both greys.

It is remarkable in tissue paper experiments how very faintly perceptible the ground colour may be and yet give effective contrast on a black or dark grey patch placed below the paper.

I believe the main factor in these experiments with faint colours is psychological, and depends almost entirely on the delicacy of our discrimination of faint distinctions of hue, and the comparative vagueness of our perception of absolute colour, especially in the pale tints approaching pure grey. I have already alluded to the illustration of this factor in experiments on complementary colour, while the colour-top and tissue paper experiments give interesting illustrations of it, apart from the production of contrast effects.

Thus, if I cover the left half of a rectangle with blue and the right half with red, I can so cover each half with tissue paper as to make its colour quite unrecognizable when seen alone with the other half concealed, and yet easily identifiable when both halves are visible at once.

It is experiences with simultaneous contrast of this type which partly justify Helmholtz's remark that contrast is most manifest with faint colours, and yet I do not think these appearances are inconsistent with the conclusions of Hering and others that in quantitative experiments the greater the saturation of the inducing field, the greater is the measured amount of contrast.

In experiments with the use of ground glass only, or equally transparent tissue paper, I think the advantage in contrast effect of the tissue paper is to be explained on that principle of the recognition of normal colour which I have argued explains the apparent advantage for the same purpose of the Ragona Scina apparatus. When a comparison between a grey above and a grey below the ground glass is made, advantage is seen to be only apparent, and whether the result of the comparison is thought to confirm or disprove the physiological explanation seems to depend largely on the personal equation of the observer. There is, I believe, a real conflict in such cases between the perception of contrast and the action of the principle of the recognition of normal colour, which leads different observers to different results.

I may add that when the double V's are used above and below the tissue paper, no differences are found by *any* observer in the result of the experiment.

11. SUMMARY.

To sum up the points we have been considering:

A. As to the reality of the physiological factor in simultaneous colour contrast:

(1) No *direct* light on this factor is thrown by experiments involving long fixation, as simultaneous colour contrast is an instantaneous effect diminished by long fixation.

(2) That the physiological factor is really operative is proved by comparative experiments as used by Hering for the case of light and dark, and by the use of the double V's and the electric torch experiment in this paper.

(3) Rollet's theory, explained and illustrated by McDougall, gives a reasonable explanation of the phenomena.

(4) Allen's extension of the induced effects to the senses of hearing and touch shows that in cases of fatigue they are a general property of nerve tissue and removes any *a priori* objection to their extension to instantaneous cases, whatever theory of vision we adopt.

B. The psychological factors:

(1) The principle of the recognition of normal colour, called by Hering "die Konstanz der Sehdinge." It operates in normal cases to conceal the induced effects, and the classical experiments are various devices to avoid its operation. It would be difficult to explain its action in normal cases if the induced effects were a direct action on the retina, as Green assumes was Hering's opinion, but if both McDougall and Allen are right in holding that they depend upon the central cortex, their disappearance in many cases is comprehensible.

(2) The principle of the relativity of our perception of visual effects, and our uncertainty as to absolute hue. This is specially stressed by Helmholtz and by Green. It is chiefly operative with faint and delicate tints and appears to be the principal factor in the tissue paper experiment.

(3) The action of suggestion. This is fully discussed by von Kries, and is not the special subject of this paper, but it is probably one of the factors operating in the coloured lights experiment.

(*Manuscript received* 20 *June*, 1928.)

QUANTITATIVE MENTAL ESTIMATES OF LIGHT AND COLOUR

BY LEWIS F. RICHARDSON.

1. *Introduction* (p. 27).
2. *Numbering browns* (p. 28).
3. *Locating pink on a mental scale of redness* (pp. 29–36).
    (1) *Preparations and methods* (pp. 29–32).
    (2) *Tabulated estimates* (pp. 32–33).
    (3) *Causes of difficulty and of variation* (pp. 33–36).
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5. *Acknowledgements* (pp. 36–37).
6. *Summary* (p. 37).

1. INTRODUCTION.

PSYCHOLOGY will never be an exact science unless psychic intensities can be measured. Some authorities say that such measurement is impossible*. Many estimates of the intensity of auditory imagery have been made by the present author†, but some psychologists were incredulous. A research was therefore pursued, in collaboration with J. S. Rose, M.A., on Loudness and Telephone current‡, and is found to show that the estimates were significant, the mean uncertainty of an individual estimate being the ratio 1 : 1·6. In the present paper the enquiry is extended to visual sensations, the question being: How do people agree or differ in their estimates of visual sensations? A further paper by R. S. Maxwell, M.A., deals with the functional relation between stimulus and red sensation. Guesses at mid-tints have been studied statistically by Karl Pearson§ some thirty years ago.

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* W. James, *Principles*, 1901, I, 546; also C. S. Myers, *Textbook of Experimental Psychology*, 1925, I, 252. But contrast Brown and Thomson, *Mental Measurement*, Ch. I.

† L. F. Richardson, "Conation, Imagery and Cerebral Conductance," *Journal of General Psychology* (Clark University Press, U.S.A.), April 1929.

‡ L. F. Richardson and J. S. Ross, "Loudness and Telephone Current," accepted by aforesaid journal.

§ Karl Pearson, "Contributions to the Mathematical Theory of Evolution," *Phil. Trans.* A, CLXXXVI, 392.

2. NUMBERING BROWNS.

1927, Oct. 11. On a rectangle of brown paper, 54 × 39 cm., were mounted four paper squares each 14 × 14 cm. and severally white, buff, brown and black. The whole was set on a drawing-board and illuminated by gas-filled tungsten lamps, shining nearly normally on it. It was arranged to be viewed from a distance of about 140 cm., and rather from one side.

Thirty-six men, studying for the Intermediate B.Sc., at Westminster Training College, were given, during an examination in practical physics in which each student did in turn the same 36 tasks of four minutes each, the following written instruction:

"Stand in the chalk ring and look at the drawing-board. On a scale of lightness and darkness, call the black square 0, the white 100. What numbers do you then give to (*a*) the background, (*b*) the brown square, (*c*) the paler square?" The time allowed to each man was four minutes. They were kept in ignorance of each other's opinions till all had been recorded. As far as I could ascertain, no one of them had had any previous training or practice in the assignment of numbers to tones. No one complained that the task was impossible; but one man put down sizes by mistake.

The paper was then covered to protect it from dirt and light.

1927, Oct. 15. The same men being seated in a class-room, I said to them that about half the class had been of one opinion as to the order of the tones and about half of the other; and that I should like to have their considered judgment. The board was mounted in view of all and the room illuminated by gas-filled tungsten lamps, daylight being excluded. On the blackboard I wrote *o* black, *a* background, *b* brown square, *c* paler square. I also told them to look at the contrasts.

At this second attempt they all gave the same sequence except one man, and when questioned he immediately conformed.

The results are set out below.

		a		*b*		*c*
Oct. 11.	Means for the 21 students who maintained the same sequence on Oct. 15 ...	34·5		52·9		86·8
Oct. 15.	For all 36 students:					
	Means	29·3		50·4		83·8
	Least	15		30		60
	Greatest	55		70		96
	Standard deviation from mean	12		12		11
			a, b		*b, c*	
	Correlation coefficients (product moment)		0·7		0 6	
	Probable error of correlation		0·06		0·07	

3. LOCATING PINK ON A MENTAL SCALE OF REDNESS.

(1) *Preparations and methods.*

This enquiry was provoked by the remark of William James: "To introspection, our feeling of pink is surely not a portion of our feeling of scarlet[*]."

The *white* used was that of a pure white cardboard sold for mounting pictures. The *scarlet* was Messrs Winsor and Newton's 'scarlet poster card.' It nearly matches the scarlet variety of mercuric iodide.

The pink was provided in different ways in the apparatuses known as 'Triangles' and 'Wheel.'

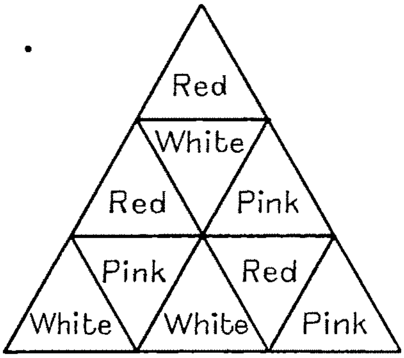

Fig. 1.

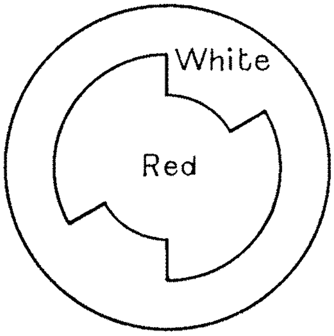

Fig. 2.

Triangles. Pale red drawing paper, sold by the Old Water Colour Society, was cut into equilateral triangles 13 cm. in the side, and was mounted with equal scarlet triangles on the white card in the pattern shown in Fig. 1. This pattern has three advantages: (i) the white, pink and scarlet occupy equal areas; (ii) each is in contact with the other two; (iii) such contacts are triplicated so as to help the observer to eliminate any slight variations of illumination across the pattern.

In a separate experiment the pink paper was compared with mixtures of the white and the scarlet on a colour wheel. It matched for darkness when the angle of red was 210, 217, 210, 245[†], 245[†] by electric light, 205 by daylight, the rest of the 360° being white. When thus matched for darkness the pink paper matched also for saturation but was decidedly too yellow.

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[*] W. James, *Principles of Psychology*, 1901, I, 546.

[†] 245 by a poster artist, Mr Clifford Gabriel; other estimates by author. We agreed about saturation and yellowness.

(Note added 1928, Dec. 21.) The reflectivities of the white, pink and red corners of the set of triangles shown in Fig. 1 have now been measured on a photometer for six ranges of wave-length. As a standard white, a slab of plaster of Paris was prepared. It was cast on plate-glass from powder sold by the British Drug Houses, Ltd.; the dark spots were cut out and refilled; and the polish, visible at nearly grazing incidence, was removed by fine emery cloth. The plaster was dried before use. This white surface, *A* in Fig. 3, was set up at one end of a photometer-bench and was illuminated by light diverging from *C*, a hole 0·2 cm. diameter, on which light from a white hot tungsten ball was concentrated by lenses. The cone of diverging light was so narrow that scarcely any of it fell on the black materials which enclosed the apparatus. The triangle for test was placed at *B* on a stiff mount. The back *D* of the mount was covered with black paper; so were also the five triangles not in use. The triangle and the plaster were viewed by an eye *E*, through a colour filter *F*, from near the hole *C*. To get the viewpoint near enough to *C* it was necessary to use the reflecting prism *G*, when *B* was near *C*. The distance *AB* was varied until *B* matched *A*. The reflectivity of *B* relative to that of *A* was then assumed to be $(BC/AC)^2$ in which *BC* and *AC* are distances. The range of wave-lengths transmitted by the filter was measured separately by a spectrometer.

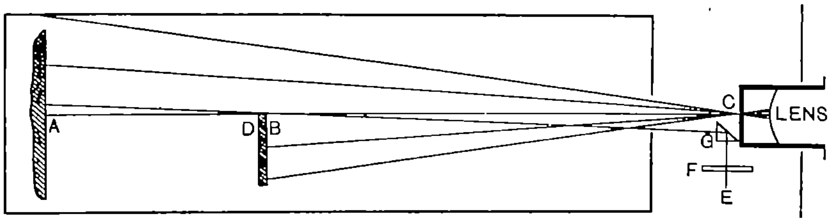

Fig. 3. Diagram of Photometer.

The results were as follows:

		Reflectivities if plaster has unit reflectivity			
Colour	Range of wave-lengths in microns	White mounting board	Pink paper O.W.S.	Scarlet poster card W. & N.	$\frac{210z+150x}{360}$
		x	y	z	
Blue	0 42 to 0·48	0·81	0 48	0·11	0·40
Green	0·47 to 0·54	0·83	0·46	0·08	0·39
Green	0·51 to 0·58	0·84	0·50	0·09	0·40
Orange	0·59 to 0·60	0·86	0·59	0·32	0·55
Red	0·61 to 0·67	0·87	0·79	0·72	0·78
Red	$0{\cdot}65_5$ to $0{\cdot}69_5$	0·91	0·77	0·76	0·82

The uncertainty of the measured reflectivity is about ± 0·02. In connection with the colour-wheel estimates, the function $(210z + 150x)/360$ is given in the last column; it should be compared with y. The excess of orange and green in y is obvious.

Wheel. The pink is here made by rapid alternations of the scarlet and the white on a disc driven by an electric motor at a speed sufficient to prevent any flicker. The white and scarlet were nominally the same as those used for the triangles. Fig. 2 shows the disc. In the middle zone, the angle occupied by scarlet was 240° ± 0·5°; that is $\frac{2}{3}$ of the whole. The advantages will be discussed later.

Procedure and instructions. Those about to observe the wheel were carefully kept in ignorance of the angles of red and white, and were allowed to see the wheel when it was rotating only.

Whereas for the browns, the observers were asked to assign numerals, for the pinks they were asked to divide a segment of a line 100 mm. long. This plan was suggested to me by Mr Ralph D. Richardson to make judgment easier, and I believe it does so. The typewritten instructions were as follows:

Comparing colour intervals.

Please look at the white, pink and scarlet (triangles). In degree of redness is the pink nearer to the white or to the scarlet?

Below is a line marked 'white' at one end and 'scarlet' at the other. Please put a mark on this line to show where you feel that the pink comes on this scale of redness.

WHITE ——————————————————————— SCARLET

Sign here   Date

Daylight }
Electric } cross out one.

If the observer showed difficulty in understanding the typed instructions, a verbal explanation was sometimes added to the following effect: "If the pink looks almost the same as the white, put a mark here (pointing to end of line labelled white). If the pink looks almost the same as the red, put a mark here (pointing to end of line labelled scarlet). If the pink is equally like the red and the white, put a mark in the middle of the line. But it is your own opinion that I want. As far as I know, there is no 'right answer.' Everyone's opinion is equally good."

If the observer put the mark very far away from the rest of the group I sometimes discussed the matter further and was surprised to find that these extreme estimates were generally not due to any misunderstanding of instructions. One paper was self-contradictory and was rejected.

Conversion to numbers. Throughout the scale is

White = 0 Scarlet = 100

The 'estimate' for the pale red is defined to be the percentage of the length of the line from the end marked white up to the mark. The lengths were measured by me, using a scale engraved on glass. Throughout the arithmetic, numbers are rounded off to the nearest unit percentage; or if exactly midway, then to neighbouring integer which is even.

(2) *Tabulated estimates.*

Table I. *Triangles.*

		Men									Women			
Group reference ...		1	2	3	4	5	6	7	8	9	10	11	12	13
Daylight or electric ...		*d*	*d*	*e*	*d*	*e*	*d*	*d, e*	*e*	*d, e*	*e*	*d*	*e*	*d*
No. who tried and failed		0	0	0	1	0	0	0	2	0	1	1	0	0
No. who made estimates		33	15	25	8	9	8	4	9	11	25	10	6	6
Estimates	Greatest ..	46	40	76	59	67	33	21	51	39	60	76	52	35
	Median ...	36	33	37	34	34	26	18	29	32	36	26	34	28
	Mean ...	36	30	40	34	37	26	17	30	30	37	36	36	26
	Least ...	20	12	25	19	13	18	12	8	17	14	20	25	14
Standard deviation of individual from group-mean		6	7	11	11	17	5	3	10	6	11	20	9	7

Group index:

Men
1. Class for Intermediate B.Sc.
2. Class for Diploma in Education
3. Class for drawing (Board of Education)
4. Teaching Staff.

(1.–4.) Westminster Training College, London.

5. Quakers. Social gathering. London, N.W. Ages 20 to 70.
6. Art students. Roy. Coll. Art. Teachers' Course, Post Diploma.
7. Other teachers and inspector of drawing and painting.
8. Fellows of the Royal Society.
9. Unclassified; mostly university graduates.

Women
10. Quakers, the same as 5, but women.
11. Art students, the same as 6, but women.
12. Unclassified, mostly relatives of group 9.

13. Class in Advanced Psychology Univ. Coll. London. Mixed sexes.

Supplementary Index for Wheel:

Men	14.	Class for Intermediate B.A., Westminster Training Coll., London.
Women	15.	Wesleyan Missionary Society's Garden Party, London.
Men	16.	Unclassified men.

Table II. *Wheel.*

	Men								Women	
Group reference	1	14	2	4	6	7	8	16	11	15
No. who tried and failed	0	0	0	0	0	0	1	0	1	0
No. who made estimates	29	16	18	6	8	2	12	20	10	36
Estimates: Greatest	58	62	83	63	74	59	78	65	89	86
Estimates: Median	40	40	40	48	52	.	35	42	66	44
Estimates: Mean	39	41	46	46	54	.	44	43	59	47
Estimates: Least	26	29	29	24	37	37	24	27	24	15
Standard deviation of individual from mean of group	7	9	15	14	11	.	15	10	20	16

Note. The same individual is counted once only for the triangles and once for the wheel, even if, logically, he belongs in several groups.

For those who are interested in frequency-distributions the following summary is collected in ranges of 5 units.

Table III. *Frequencies.*

Centre of range of estimate	Triangles excluding group 13: Men	Triangles excluding group 13: Women	Wheel: Men	Wheel: Women
5	0	0	0	0
10	3	0	0	0
15	2	2	0	1
20	11	4	0	0
25	13	5	4	2
30	24	4	17	3
35	34	10	19	9
40	21	7	29	4
45	6	1	11	4
50	2	5	5	6
55	1	0	8	1
60	2	1	9	1
65	2	0	4	5
70	0	0	2	5
75	1	2	1	3
80	0	0	1	0
85	0	0	1	1
90	0	0	0	1
95	0	0	0	0
Total persons	122	41	111	46
Mean estimate	34	36	43	50
Standard deviation	10	13	12	17

(3) *Causes of difficulty and of variation.*

Observers have mentioned many causes. Although I have not thoroughly explored any variations, the following notes may be useful to future investigators.

(1) *Obliquity of view.* Care was taken to choose papers equally matt, so that the pink seemed to me in about the same relation to white and scarlet from whatever angle the card was viewed.

(2) *The change from daylight to electric,* might be expected to affect

the triangles. The means are: daylight 36, 30, 34, 26, 36, mean 32·5; electric 40, 37, 30, 37, 36, mean 36, and the difference is scarcely significant. With the colour wheel any such effect is probably less.

(3) *Confusion between darkness and saturation.* Several subjects were perplexed about this for the triangles. Some observers stated that they estimated by darkness, but I have not rejected their estimates on that account. With the wheel the estimate would probably be much the same whether judged by darkness or saturation.

(4) *Pink not of same hue as scarlet.* The *Concise Oxford Dictionary* describes pink as a "pale red slightly inclining to purple," and scarlet as a "brilliant red colour inclining to orange." Thinking that these contrary inclinations might perhaps have been all or part of the difficulty that James felt, I tried, in choosing the pale triangles, to make them of the same hue as the red. But evidently I overshot the mark, for many subjects objected, stating that the 'pink' was apricot, buff, brownish, flesh coloured, terracotta, too yellow, too opaque, too grey. Indeed two observers in Group 8, who said that the task was impossible, gave this difference of hue as their reason.

The colour wheel was made to avoid this difficulty and at the same time to give much the same result whether the observer judged by saturation, as the instructions indicate, or by darkness. Yet even when the pink was made by mixing the white with the scarlet on a colour wheel an Art Student remarked "pink is slightly purple—probably slightly off the true scale" and six others in the same group made similar if less explicit comment. One other subject said that the pink was too blue.

These experiences suggest that people vary a good deal in their sensitivity to light waves of lengths between 0·6 and 0·7 micron.*

(5) *Half closing the eyes.* Two observers stated that this moved the pink nearer to the white.

(6) *Distance.* Two skilled observers remarked that the estimate increased with distance, roughly thus:

	1 m.	2 m.	6 m.
Triangles, electric light ...	15	—	21
Wheel	25	36	49

But on sorting the estimates of Group 1 on triangles any effect of distance was lost in the general scatter. The distances used were between 1 and 5 metres.

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* As is well known. See Sir J. Herbert Parsons, *An Introduction to the Study of Colour Vision* (Cambridge University Press), 2nd ed. p. 193.

(7) *Bias by pigments.* One observer, who gave the lowest estimate in Group 5, remarked that he was a dyer and was influenced by a knowledge of the strength of dye required to make the paper. Accordingly I asked the Art students to put from them all thought of quantities of pigment and to judge the colours simply mentally. But, having tried, about half of them protested that they could not thus put aside their experience.

(8) *Practice.* Groups 6 and 7 have of course had most practice, but also most opportunity for bias by pigments. No one remarked on having tried this sort of task before; and I provided no practice beyond the repetitions here recorded. That is to say, the group that made most repetitions was No. 1, the Intermediate Science class, and their successive standard deviations were:

1927 Oct. 11 Browns	1927 Oct. 15 Browns	1928 June 11 Pink wheel	1928 July 12 Pink triangles
Confused	12	—	—
	12	6·$_6$	6·$_2$
	11	—	—

Much practice would be a desirable preliminary to future researches.

(9) *Quick changes.* One observer of the triangles marked his first impression as 8, changing during about three minutes to 26.

Another estimated the triangles at 29 by first impression and added that "reasoning would tend to bring the mark half-way—if not nearer to scarlet."

Another said of the wheel that the pink "appeared to become paler the longer the colours were viewed."

Another agreed with this last statement.

(10) *Abnormalities of colour vision.* That eyes may differ is shown by an educationalist, aged 77, who viewed the triangles by electric light and reported that by right eye he saw scarlet and gamboge; estimate 27. By left eye, which is partly blind to red, he saw brick red and lemon yellow; estimate 13. Other estimates by men who said that their vision was peculiar were:

	Triangles		
Abnormality	daylight	electric	Wheel
Unknown type ...	—	26	—
Unknown type ...	38	—	31
Blind for red ...	—	34	—
Weak for red ...	—	—	24
Weak for green ...	—	—	30

(11) *Sex.* The means for men and women are shown separately at the foot of Table III. On the wheel women see the pink redder than do the

men and the difference is perhaps significant, for the means are 43 ± 0·8, 50 ± 1·7 where the numbers following the ± sign are 'probable errors.'

(12) *Age.* The observers mentioned in Tables I, II, III were all over 17. A few children have also looked at the triangles and at two other wheels. Below 12 years, estimates were sometimes very high, as if erratic.

(13) *Persistence of individual differences.* This was sought for by plotting correlation diagrams in which the co-ordinates of a point are the estimates of the Triangles and of the Wheel made by the same person. This was done for Groups 1, 2, 6, 11. But there was no significant correlation. We might expect to find persistent individual differences if the observers had much more practice. For the browns there was the significant correlation stated in § 2.

(14) *Reasoning.* Mr Dudley Heath made the interesting suggestion that those art students who decline to make a decision in this and similar problems are often those who are also argumentative; and that they could place the mark on the line if they would trust to their first impressions, instead of to reasoning.

4. REPLY TO AN OBJECTION.

The quotation from James continues "...nor does the light of an electric arc seem to contain that of a tallow candle in itself." Even if we could abstract brightness as a quality of the light, it would be difficult to compare the brightnesses, because the arc is dazzling.

Similarly it is difficult to compare the loudness of a pistol-crack with that of a cannon-roar[1] because the roar is deafening.

Here is another specimen. It is very difficult to say how many times less painful it is to be pricked by a pin than to have a tooth extracted without anaesthetic; because the dental pain is overwhelming.

But these are no arguments against the estimation of less violent sensations.

James in this passage was perhaps more concerned to emphasize the words *portion* and *contain*. That is another question. There is no evidence that any of the present observers counted up little bits of pinkness, and no objection to James's remarks about addition.

5. ACKNOWLEDGEMENTS.

My best thanks are due to the many observers for their careful opinions; and also especially to those who helped to organize the work,

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[1] C. S. Myers, *Textbook of Experimental Psychology* (1925), Part I, 252.

namely Dr H. H. Dale, Sec. R.S., Prof. Flügel and Mr Raper of University College London, Mr Wellington and Mr Dudley Heath of the Royal College of Art, and to Principal Workman, Mr Patten and Mr J. S. Ross of Westminster Training College.

The arithmetic was performed on a machine lent by the Government Grant Committee of the Royal Society.

6. SUMMARY.

(1) Of 164 persons who were asked to estimate quantitatively the redness of pink paper, only five said that the task was impossible. With a pink made on a colour wheel only two out of 159 found the task impossible.

(2) The pink, made by mixing $\frac{2}{3}$ of scarlet with $\frac{1}{3}$ of white on a colour-wheel, was judged mentally to be at the following positions on a scale in which 0 is white, 100 is scarlet. Mean estimate by men 43, by women 50. There is thus a remarkable disagreement between angles and mental estimates. Mr R. S. Maxwell, M.A., is exploring this further.

(3) The standard deviation of an individual estimate from the mean for the same sex is about 13 of these units. Many possible causes of this variability have been discussed.

(*Manuscript received* 16 *August*, 1928.)

A FURTHER EXPERIMENT ON AUDITORY FATIGUE

By FRANK A. PATTIE, Jr.

(*From the Psychological Laboratory, Harvard University.*)

1. *Discussion of earlier work* (pp. 38–39).
2. *New experiment described* (pp. 39–41).
3. *Results* (p. 41).
4. *Conclusions* (pp. 41–42).

1. Discussion of earlier work.

In 1920 J. C. Flügel[1] reported experiments on the shift of localization of a binaural tone which occurs after 'fatigue' of one ear by stimulation with a continuous tone. The apparent localization is shifted towards the unfatigued side. In this paper this effect will be called 'localization-fatigue.' In 1927 the writer[2] reported experiments, following investigations by Flügel and others, on the diminution of intensity of tones after prolonged stimulation. This effect will be referred to as 'intensity-fatigue.'

In 1922 F. C. Bartlett and H. Mark[3] performed an experiment in which they obtained the same shift of localization as in Flügel's work, but after a period of binaural stimulation in which the tone conducted to one of the ears was leading in phase with respect to the other tone. It seemed to the subjects that they were being stimulated in only one ear, that in which the phase was leading, and the 'localization-fatigue' occurred just as it would have occurred if they had been stimulated in that ear only. These investigators attribute the effect not to local fatigue but to an "error of judgment." "The extreme right or left position having been presented for some time, a 'set' is established as a result of which a marginal position appears more central than it normally does." Flügel would perhaps not be unprepared for such an experimental result, for he distinguishes "fatigue of the process of binaural localization" and the fatigue which causes diminution of intensity, and his data for localization-fatigue vary so widely among themselves as to suggest the participation of psychological factors in the production of this effect.

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[1] "On local fatigue in the auditory system," this *Journal*, xi, 105–134.

[2] "An experimental study of fatigue in the auditory mechanism," *Amer. J. Psychol.* xxxviii, 39–58.

[3] "A note on local fatigue in the auditory system," this *Journal*, xiii, 215–218.

The experiment of Bartlett and Mark did not rule out the possibility of a differential intensity-fatigue, which might have been produced in the two ears because of the difference in phase of the two tones. The results of the work of von Hornbostel and Wertheimer[1] and of Klemm[2] (which showed that if two clicks, qualitatively alike but separated by a very short interval of time, are presented separately to the two ears only one click is heard and its localization is on the side of the earlier) and the facts of the effects of binaural phase differences on localization of tones led the writer to expect that a different degree of intensity-fatigue would be shown if the ears were stimulated as in Bartlett and Mark's investigation and the intensities of a tone as perceived in each of the ears were afterwards compared. The experiments on the localization of clicks and those on binaural phase relationships as determinants of localization may be considered as two cases of one phenomenon, namely, the inhibition of a later excitation by an earlier, if we assume that there are certain maxima of excitation produced in the auditory nervous mechanism which correspond to certain periodic features of the tonal vibration. If the series of later excitations is centrally inhibited by the earlier, it seems reasonable to expect that the physiological effects of the later excitations should be abolished. The effect of continued stimulation is a diminution of tonal intensity, and it is conceivable that this effect might vanish in that ear which is stimulated by the tone which lags in phase behind a tone presented to the other ear. This hypothesis assumes that the seat of intensity-fatigue is central rather than peripheral, but at present we are in ignorance as to the actual state of affairs; Bleyl[3] suspected that it was central, for he found that his neurasthenic subjects were unusually fatigable when he gave them tests with tuning forks, and he assumed that the general fatigability associated with neurasthenia was central in origin.

2. NEW EXPERIMENT DESCRIBED.

In the experiment designed to test this hypothesis on the relationship between binaural phase differences and intensity-fatigue, two kinds of stimulation were employed alternately in every experimental sitting.

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[1] *Sitzungsber. d. preuss. Akad. d. Wissensch.* 1920, 388–396.

[2] *Arch. f. d. ges. Psychol.* 1919, XXXVIII, 105–110; 1920, XL, 117–146.

[3] *Arch. f. Ohren-, Nasen-, u. Kehlkopfheilk.* 1921, CVIII, 192–197.

The subject was first stimulated binaurally with two tones which were out of phase to such an extent as to make the localization appear wholly on the side of the tone with leading phase. After this type of stimulation, in which the subject thought he was being stimulated uniaurally, the condition of the ears as to intensity-fatigue was tested. If intensity-fatigue should be found in the ear on the side of the leading phase and not in the other ear, the hypothesis would be verified. The subject was then stimulated uniaurally on the side on which the phase had led in the period of binaural stimulation, and the degree of intensity-fatigue of the stimulated ear was then measured; this second kind of stimulation was used in order to make sure that the binaural tones were strong enough to produce intensity-fatigue when acting singly. These two types of stimulation were called *A* and *B*, respectively; a more detailed description of them follows:

A. Binaural diphase stimulation for 1 minute with a loud tone of 512 volts produced by a Western Electric 2-A audiometer, amplified by means of a Western Electric 25-A amplifier, and conducted from a telephone receiver in a sound-proof box through a Y-tube, the branches of which were connected to rubber tubes leading to the subject's ears. The loudness of the tone at the receiver was 130 'sensation units.' The length of tubing from the source to the ear was about 6·3 m. The diameter of the tubing was 1·5 cm. The tube leading to one ear was lengthened so as to throw the tone in that ear approximately half a wave-length behind the other tone. Localization of the binaural tone was entirely on the side of the leading phase. This change in phase did not cause any appreciable difference in the intensities of the two tones.

B. Uniaural stimulation for 1 minute from the same source and with the same conduction as in *A*, but with the connection broken between the branch of the Y-tube and the rubber tube which in *A* conducted the tone of lagging phase. The branch of the Y-tube was stuffed with cotton wool; in a later technique it was connected to a rubber tube, plugged at its end with cotton wool, which was as long as the tube leading to the ear. This method was used in order to make the uniaural stimulus strictly identical with the stimulus on the same side which was acting when *A* stimulation was employed.

A and *B* types of stimulation were employed alternately until each had been used ten times in the experimental sitting.

The test for intensity-fatigue was made according to the writer's method described in the paper already cited. Two tones, very near subjective equality when the ears are normal, were successively presented, one in each ear, for comparison after the minute-long stimulation. Intensity-fatigue in one ear will be shown by an increase in the number of judgments of 'less' and a decrease in the number of judgments of 'greater' given with respect to the tone presented in the 'fatigued' ear.

3. RESULTS.

The results of the experiment showed that binaural diphase stimulation does not produce any differential intensity-fatigue; both ears are equally fatigued by such stimulation. The hypothesis is therefore not verified, yet the data are nevertheless valuable, for certain conclusions may be drawn from them. The three subjects used gave the same results. A typical set of data obtained from two hours' work with one of the subjects follows:

1. *A* stimulation with phase leading in the left ear. The two comparison-stimuli used to test for intensity-fatigue were presented in the order left–right, and the subject reported whether the loudness of the second tone was greater than, equal to, or less than that of the first; hence, if the left ear had been fatigued to a greater extent than the right, there would be an unusually large proportion of 'greater' judgments soon after the stimulation. The results are:

	Number of judgments out of a total of 20 at					
	5	10	15	25	35	45 seconds after *A* stimulation
Greater	2	4	3	4	3	7
Equal	8	8	7	8	8	8
Less	10	8	10	8	9	5

2. *B* stimulation of the left ear for 1 minute. Same presentation of comparison-stimuli. Fatigue of the left ear is shown by the distribution of 'greater' and 'less' judgments and the decrease in the number of the former, accompanied by an increase of the number of the latter, with passage of time.

	Number of judgments out of a total of 20 at					
	5	10	15	25	35	45 seconds after *B* stimulation
Greater	15	13	12	7	5	1
Equal	2	4	3	4	2	9
Less	1	3	5	9	13	10

4. CONCLUSIONS.

The conclusions that may be drawn from the experimental data are these:

1. Bartlett and Mark's experiment is freed from the objection that they had not ruled out the possibility that their method of stimulation might have produced a differential intensity-fatigue such as was looked for in the present investigation. Although we knew that it requires a considerable binaural difference of intensity to cause a shift of localization from the median position, we did not know that a binaural intensity-difference might not be at least a co-operating factor in their experimental situation where localization-fatigue was produced.

2. We have some ground for believing that the seat of intensity-fatigue is peripheral and not central. The argument, which is *formally* valid, is: If intensity-fatigue is central rather than peripheral in origin, differences of phase will produce a differential fatigue-effect in the two ears: but such an effect was not produced; therefore, the fatigue is not central.

3. Subjective factors have nothing to do with intensity-fatigue. In 1908 Sewall[1] performed experiments which led him to think that intensity-fatigue was entirely conditioned by attention. In the writer's former study of it, he attempted to demonstrate the irrelevancy of subjective factors by the greatest possible variation of the experimental method, but he was not able to *prove* it. In this experiment the subjects thought they were being stimulated in only one ear; therefore, the subjective conditions of the former experiments with uniaural stimulation were exactly realized, yet fatigue was produced both on the side on which the stimulus was perceived and also to an equal extent on the side on which no stimulus was perceived. (There is an inescapable consideration which must be mentioned here: it is merely assumed, because of the principle of continuity, that both ears were *fatigued* by the binaural stimulation. As a matter of fact, the results show only that both ears were in the same condition with respect to sensitivity. We have no way to determine whether they were fatigued or not, since we have no third ear which could be kept isolated from stimulation and used as a standard for the measurement of fatigue in the other two. There is no reason, however, for doubting that they were in a fatigued rather than in an unfatigued condition.)

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[1] *Zeitschr. f. Sinnesphysiol.* 1908, XLII, 115–123.

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A FURTHER STUDY OF THE PSYCHOLOGICAL EFFECTS OF LONG SPELLS OF REPETITIVE WORK

By K. G. POLLOCK.

(*From the Cambridge Psychological Laboratory.*)

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I. INTRODUCTION.

SEVERAL attempts have been made to investigate the relation between accuracy and speed of work, and duration of work. The results so far have been extremely variable, tending to show that the relationship depends on individual factors and the nature of the work, rather than on any general principle. This paper records a further attempt towards the elucidation of the problem and the results must be considered together with those of previous experiments.

II. EXPERIMENTAL WORK OF PROF. B. MUSCIO, DR G. TERUOKA AND MISS VERNON.

Prof. B. Muscio[1], experimenting in 1922 with an Aiming Test and a Pendulum Test, concluded that inaccuracy in this type of work was a function of the rate rather than of the duration of work. Dr G. Teruoka[2], carrying out a further investigation with the Aiming Test, also found that increased speed and decreased accuracy were associated, but that after about two hours' work at any particular rate, there was a decrease in accuracy. Some further work on the Pendulum Test was done by Miss M. D. Vernon[3]; the results, although indefinite, were not in agreement with those of Muscio, as there was a gradual decrease in accuracy after the first hour of work. She then employed a Paper-folding Test and a Maze-tracing Test, and observed a decrease of output in the afternoons in both tests, and an increase of errors in the Maze-tracing Test. Thus in all of these experiments there is some evidence for decreasing quality of work with long spells, particularly with periods of about two hours' duration, but there is little or no agreement with regard to the causal factors in operation.

III. DESCRIPTION OF GUIDIT EXPERIMENT.

Since the Paper-folding and Maze-tracing Tests were troublesome to score, a new test was employed in the experiment described below. The apparatus was that of a game called 'Guidit[4],' and the task consisted in guiding a small metal ball up an inclined plane with a fine knitting-needle, set in a thin handle. The plane was a wooden board, 13 inches

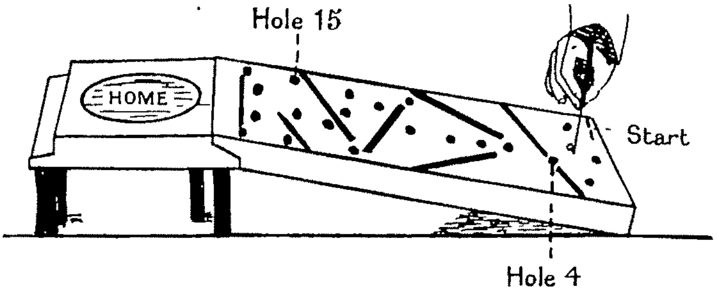

Fig. 1.

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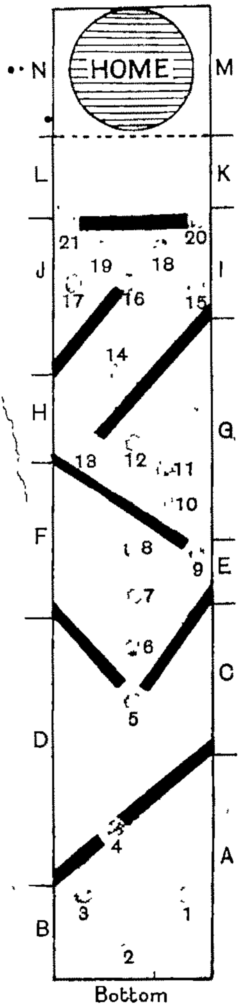
[1] *Report of the Industrial Fatigue Research Board*, No. 19 (General Series, No. 7), B.

[2] This *Journal*, XVI, 223.

[3] This *Journal*, XVI, 222–236.

[4] British manufacture. Maker unknown.

long by 3½ inches wide, and inclined at an angle of about ten degrees to the horizontal. There were 21 holes in the board large enough for the ball to drop through and also seven barriers to be circumvented. The subject sat at a table, holding a stopwatch in his left hand, and after every attempt wrote down the place reached and the time taken. A system of marks was employed in order that some credit might be given when the ball, although not reaching the top, was manœuvred a certain distance. When the ball was taken to the top, 10 marks were scored and this was



Home. 10 marks

Holes	Marks	Side portion	Marks
1	0	Bottom	0
2	0	A	0
3	0	B	0
4	1	C	2
5	2	D	2
6	3	E	3
7	3	F	3
8	3	G	4
9	3	H	5
10	4	I	7
11	4	J	7
12	4	K	8
13	5	L	8
14	6	M	8
15	7	N	8
16	7		
17	7		
18	7		
19	7		
20	7		
21	7		

Fig. 2. Plan of Guidit board showing system of marking.

called a 'success.' The rest of the scale was compiled after a few trials to determine as far as possible the relative difficulties at different points of the board. The scale is given in full on the preceding page (see Figs. 1 and 2).

Three subjects learned to perform the task, but one only performed the long spells, and this paper deals with her results alone. The subject started with practices of half an hour's duration and found that at first 40 attempts were made in this time. She then adopted a set of 40 attempts as a unit, and during the practice period worked sometimes one and sometimes two of these sets every day except Sundays and a few other holidays. Maximum accuracy was reached in the 29th set but was not sustained. After the 35th set, a vacation of six weeks intervened before the 36th, and the work immediately after the vacation was less accurate than before, accuracy improving up to the 47th set. After the 51st set she worked for six periods of about one hour, two sets of 40 attempts being worked in each period. They were performed in the mornings, four on consecutive days and the last two after an interval of one day. Six periods of two hours and six of four hours were then performed in the afternoons of three consecutive days in each of four weeks. On most of the intervening days there were short practice periods. Finally six eight-hour periods were worked on six consecutive days, on each of which there were two four-hour spells, with an interval of one hour for lunch.

IV. RESULTS.

(*a*) *Accuracy and speed.*

In order to determine whether there is any significant relation between the length of the working spell and the quality of the work done, average marks obtained for the whole spell and for various sections of the spell were computed. It is convenient to combine in a single table the average marks and numbers representing the average time taken for successful attempts. A 'successful' attempt is one in which the ball is taken 'Home,' that is to the top of the inclined plane. I am assuming that the average time taken for successful attempts fairly represents the general rate of work throughout the particular spell under consideration. This may, of course, be unjustifiable, and no doubt a more accurate record of the actual normal distribution of speed in movements throughout a 'successful' run ought to be made. No record is given of the actual *number* of attempts made during each hour, or other periods less than the total, of work. Since records and introspections were taken down during the working periods, such numbers could have no important significance.

Table I.

Length of spells	Total no. of attempts		Mark whole periods	Time whole periods seconds	Mark 1st halves	Time 1st halves seconds	Mark 2nd halves	Time 2nd halves seconds
1 hr.	480	Average	8·7	19·5	8·8	20·5	8·7	18·5
		S.d.	2·2	3·7	2·0	2·2	2·2	3·4
2 hrs.	1440	Average	8·2	15·4	8·4	16·5	8·0	14·2
		S.d.	2·3	3·8	2·5	2·0	2·1	3·6
4 „	3700	Average	8·6	10·2	8·7	10·8	8·4	9·6
		S.d.	2·6	1·9	2·4	2·8	2·8	2·0
8-hr. days	7240	Average	8·4	9·5	8·6	9·6	8·1	9·4
		S.d.	2·8	1·3	2·6	2·9	2·9	1·3

It will be seen that the quality of the work as indicated by the average marks was slightly higher in the one-hour periods and slightly lower in the two-hour periods than in the four and eight hours. Speed of work, on the other hand, increased up to the four-hour periods and then remained fairly constant. It is highly probable that this increase of speed is, in the main, a practice effect, and that in this respect practice continued to influence results up to about half-way through the four-hour periods (see Table II).

Table II. *Average marks and times for success for six four-hour spells.*

Spell no.	Total no. of attempts		Mark	Time seconds
1	600	Average	8·0	11·5
		S.d.	2·7	2·5
2	600	Average	8·4	11·6
		S.d.	2·6	1·8
3	620	Average	8·4	10·0
		S.d.	2·8	1·7
4	620	Average	8·8	9·8
		S.d.	2·4	1·5
5	640	Average	8·8	9·2
		S.d.	2·5	1·0
6	620	Average	9·0	9·8
		S.d.	2·3	1·6

Table III. *Average marks for all* 1*st*, 2*nd*, 3*rd* ... 15*th sets of* 40 *attempts in eighteen four-hour spells.*

Sets of 40 attempts	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th	11th	12th	13th	14th	15th
Average mark for 18 sets	8·7	8·8	8·8	8·7	8·5	8·7	8·4	8·2	8·3	8·6	8·4	8·4	8·1	7·8	7·9
Std. dev. of set averages	0·2	0·7	0·7	0·6	0·5	0·5	0·8	0·6	0·5	0·4	0·4	0·7	0·8	0·6	0·9

Taking all the four-hour spells together (including two on each eight-hour day), average marks were highest during the second and third sets of 40 attempts. At this stage a set took between fifteen and twenty minutes to perform, and so accuracy was highest from about fifteen minutes to one hour after the start. Here again the differences are small (10 per cent. between highest and lowest), but since the difference between the best and worst single sets performed at this stage was only 53 per cent., small differences in averages are worth recording. (See Table III.)

Table IV. *Marks and times for success for six eight-hour days.*

Day	Total no. of attempts		Mark whole day	Time whole day seconds	Mark a.m.	Time a.m. seconds	Mark p.m.	Time p.m. seconds
Mon.	1200	Average.	8·5	9·6	8·8	9·9	8·2	9·2
		S.d.	2·6	1·5	2·3	1·6	2·8	1·4
Tues.	1180	Average	8·7	9·4	9·0	9·7	8·4	9·0
		S.d.	2·6	1·3	2·3	1·3	2·8	1·2
Wed.	1200	Average	8·5	9·2	8·7	9·4	8·3	9·1
		S.d.	2·7	1·3	2·4	1·2	2·9	1·3
Thur.	1240	Average	8·3	9·4	8·6	9·1	8·0	9·7
		S.d.	2·9	1·0	2·7	1·0	3·1	0·9
Fri.	1220	Average	8·2	10·1	8·3	10·2	8·1	9·9
		S.d.	3·0	1·2	2·9	1·1	3·1	1·2
Sat.	1200	Average	8·0	9·3	8·2	9·4	7·8	9·1
		S.d.	3·1	1·2	3·0	1·1	3·1	1·2

During the week of eight-hour days, accuracy was highest on the second day, Tuesday. On this day the number of attempts was smallest, but this might be expected since successful attempts naturally took longer than failures. After Tuesday average marks decreased regularly as the week went on. The afternoon marks were all lower than those for the preceding mornings and the standard deviations of marks were higher in the afternoons. On the first three and the last days, average time was greater in the mornings, on the fourth and sixth days it was greater in the afternoons.

Very broadly speaking, there appears to be a slight involuntary increase in the rate of work during the second as compared with the first half of a working spell, irrespective of the total length of the spell, and at the same time a slight decrease of accuracy. The increase in speeding (particularly in the eight-hour periods where the decrease in accuracy is greatest) is however so slight that it is rather difficult to believe that the decrease of accuracy is in any way due to this.

(*b*) *The effect of 'pauses.'*

On the eight-hour days an interval of an hour for lunch was allowed between the morning and afternoon spells.

Table V. *Average marks and time for last* 40 *attempts before and first* 40 *attempts after lunch pauses, expressed as percentages of average marks and average time for each day.*

	Percentages			
	Average marks		Average time	
	Last 40 before pause	First 40 after pause	Last 40 before pause	First 40 after pause
Mon.	99	104	85	89
Tues.	100	103	96	97
Wed.	98	104	105	105
Thur.	96	109	95	109
Fri.	97	103	105	100
Sat.	83	110	103	106
Average	96	106	98	101

It will be seen that the quality of work was, each day, higher immediately after the pause than before. Speed was greater once, equal once, and lower than before four times.

Rest pauses of five minutes with refreshments occurred about the middle of the four-hour days, and of each four-hour spell of the eight-hour days.

Table VI. *Five-minute pauses. Average marks and average time for last* 40 *attempts before and first* 40 *attempts after pauses expressed as percentages of average marks and average time for each spell.*

	Percentages			
	Average marks		Average time	
Four-hour spells	Last 40 before pause	First 40 after pause	Last 40 before pause	First 40 after pause
Spell no. 1	94	96	90	92
„ 2	93	100	91	100
„ 3	103	98	87	88
„ 4	91	100	90	94
„ 5	98	101	101	100
„ 6	96	95	89	91
Eight-hour days				
Mon. a.m.	106	96	93	97
„ p.m.	93	110	98	112
Tues. a.m.	100	96	91	102
„ p.m.	104	102	104	109
Wed. a.m.	93	99	94	99
„ p.m.	96	111	97	91
Thur. a.m.	91	98	95	96
„ p.m.	105	101	101	101
Fri. a.m.	91	107	92	98
„ p.m.	100	108	94	96
Sat. a.m.	101	103	103	104
„ p.m.	94	106	98	96
Average for 18 periods	103	99	95	98

Average marks for the first set of 40 attempts after the pause were better than for the last set before the pause twelve times and worse six times. Average time was longer fourteen times, unchanged once and shorter three times, Pearson's coefficient of correlation between increase of marks and increase of time being 0·3. For 18 pairs of observations a value less than 0·6 is scarcely significant.

There were also 42 incidental pauses lasting from one to three minutes for attention to stop watch, window, fire or electric light. Taking the averages for the 10 attempts before and after these interruptions, marks were worse afterwards 21 times, better 17 times and unchanged 4 times. Time was shorter 25 times, longer 16 times and unchanged once. Although accuracy on the whole suffered from these pauses, they were always extremely welcome, as they provided an opportunity of moving about.

(c) *The effects of feelings of tiredness.*

Feelings of tiredness were noted at certain times, but since they were not the original object of the study, probably they sometimes occurred without being recorded. During the practice and one-hour periods there was no reference to tiredness. At the end of the two-hour periods there were remarks such as 'rather tired' and 'ache in shoulders.' The subject felt tired at the end of every four-hour period (these were worked in the afternoons) and at the end of all afternoons of the eight-hour days. Tiredness was also noted at the end of the mornings of the last four days.

Table VII. *Average marks and time for 'tired' sets expressed as percentages of average marks and time for the spells in which they occurred.*

Four-hour spells	No. of 'tired' sets of 40 attempts	Percentage	
		Marks	Time
Spell no. 1	1	98	93
,, 2	2	98	94
,, 3	1	108	100
,, 4	½	95	87
,, 5	3	99	99
,, 6	2½	98	98
Eight-hour days			
Mon. p.m.	5	94	108
Tues. p.m.	4	86	98
Wed. a.m.	2	94	102
,, p.m.	3	88	100
Thur. a.m.	3	90	100
,, p.m.	5	91	101
Fri. a.m.	2½	100	101
,, p.m.	6	91	100
Sat. a.m.	7	93	100
,, p.m.	3	95	100

The average mark for all 'tired' sets at the end of spells was 7·6 or 90 per cent. of the average mark for all the four- and eight-hour periods. Average time was unchanged. (See Table VII.)

(*d*) *Effects of 'conscious attitudes.'*

A matter of great interest in all psychological experiments is the possible effect upon results of varying 'conscious attitudes' on the part of the subject. As occurring in associative and higher thought processes generally, they have received a great amount of attention, but their precise effects in relation to specific sensory and motor processes need far more detailed study. An experiment of the type which I am now discussing affords abundant opportunity for this study but my remarks in regard to this must be taken rather as indicating points upon which future observation may well concentrate, than in themselves conclusive.

(1) *Cheerfulness and confidence.* At the beginning of a spell the subject was usually cheerful and after a short practice time became confident. Average marks were then high, over 9 for from one to five successive sets of 40 attempts, that is, for periods varying in length up to an hour and a half. Average time tended to decrease after the first set. After this beginning there was no uniformity, accuracy diminished but in an irregular fashion. Often a change of mood seemed to coincide with decreasing accuracy. Cheerfulness and accurate work were certainly associated, but whether as cause and result or *vice versa* this experiment did not prove.

(2) *Devices to maintain cheerfulness.* It was found that certain devices, consciously adopted, could assist in maintaining cheerfulness. The most useful of these appears to have been singing. This started on the

Table VIII. *Average marks and time for sets during which the subject sang, expressed as percentages of average marks and time for the spells during which these sets occurred.*

Period		No. of 'singing sets'		Percentage	
				Marks	Time
Tues.	a.m.	4¼		102	102
,,	p.m.	5¼		106	113
Wed.	a.m.	4½		102	104
Thur.	a.m.	5		105	101
,,	p.m.	6¾		103	102
Fri.	a.m.	6½		105	100
,,	p.m.	4½		106	104
Sat.	a.m.	5¾		105	101
,,	p.m.	4		105	107
	Total	46½	Average	104	104

Tuesday morning of the week of eight-hour periods, and for the rest of the week, the subject sang to herself during each spell except that on the Wednesday afternoon, for periods of time varying from one to two hours.

It will be seen that during these singing periods accuracy was slightly higher and time slightly longer than the general average. But accuracy did not remain at the same level throughout a singing period, it tended to rise gradually and then fall again when the subject was getting bored with singing. Time varied irregularly. (See Tables VIII and IX.)

Table IX. *Example of fluctuations in marks and time during a 'singing' period. Friday, p.m.* (*Averages for* 10 *attempts.*)

Sets of 10 attempts	1st	2nd	3rd	4th	5th	6th	7th	8th	9th
Average marks	7·9	7·4	8·2	9·7	9·4	8·8	10·0	8·4	9·6
Average time	9·9	9·8	9·8	10·7	10·4	10·8	10·1	10·4	10·1
Sets of 10 attempts	10th	11th	12th	13th	14th	15th	16th	17th	18th
Average marks	8·8	9·1	9·1	8·4	8·0	9·4	6·9	7·3	7·2
Average time	9·8	10·3	9·5	10·1	9·8	10·1	12·0	10·4	11·8

Another device to maintain cheerfulness was definite concentration on speed, as providing a special interest. This was recorded 12 times with the following results:

Table X. *Average marks and time for periods of concentration on speed expressed as percentages of average marks and time during the spells in which these periods occurred.*

	No. of attempts in period of concentration	Percentage	
Two-hour spells		Average marks	Average time
Spell no. 5	8	102	63
,, 5	7	100	71
Four-hour spells			
Spell no. 2	10	104	86
,, 3	20	102	96
,, 4	25	105	85
,, 6	50	90	84
Eight-hour days			
Tues. a.m.	40	101	87
,, p.m.	80	93	96
Wed. p.m.	40	101	92
Thur. a.m.	40	98	87
,, p.m.	15	100	89
Sat. p.m.	40	99	86
Total	375	98	88

(3) *Boredom and annoyance.* In spite of the above devices to maintain cheerfulness, the subject was unsuccessful in amusing herself the whole time. She experienced periods of severe boredom, especially during the

eight-hour days. Three distinct types were reported: (i) angry, (ii) 'nightmare' or compulsive, (iii) passive.

(i) The subject raged at the experiment and was annoyed with herself for working badly. The mood was present on six occasions, and each time the average mark was lower than the average for the spell in which it occurred.

Table XI. *Average marks and time during 'angry' sets expressed as percentages of average marks and time for the spells in which they occurred.*

Eight-hour days			Set	Time after	Percentage Average mark	Percentage Average time
1	Wed.	p.m.	5	1¼ hrs.	92	100
2	Thur.	a.m.	7	1¾ ,,	91	93
3	,,	p.m.	15	4¾ ,,	81	96
4	Fri.	p.m.	13 and 14	4¼ ,,	88	101
5	Sat.	a.m.	13	4¼ ,,	80	105
6	,,	p.m.	2	20 min.	94	107
Average for 'angry' sets					88	100

(ii) The 'nightmare' or compulsive feeling can best be described by quoting the introspections recorded the first time it appeared. "It all feels rather like a bad dream; I have *got* to do this silly thing and go on doing it. Something outside is driving me on against my will." This feeling was present during the first set on the Friday morning and again during the fourth. It was overcome by suggestion and an effort to improve which will be discussed later. For those sets the average mark was 92 per cent. and time 100 per cent. of the averages for the whole spell. The feeling was also present during the 13th set on Friday afternoon but then changed into the angry type of boredom which is recorded above.

It is interesting at this point to compare some results of experiments by E. M. Smith and F. C. Bartlett on "Listening to Sounds of Weak Intensity[1]." Subject S reports on p. 154: "I had a fit of absolutely choking rage....This lasted for a while and I could not attend at all. There was an extraordinary feeling of being thwarted, accompanied by a horrible feeling of helplessness which took all my attention away from the sound, and would have prevented my hearing it anyway." The report continues: "In spite of these apparently unfavourable moods, the observer's performance on this occasion was approximately normal. In the Guidit experiment the subject's performance during moods of anger or 'nightmare' was definitely below normal. The difference in results may possibly be due either to the fact that a motor and not a sensory response

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[1] This *Journal*, March 1920, x, pts. 2 and 3.

Table XII. *Average marks and time during 'bored' sets expressed as percentages of average marks and time for spells in which they occurred.*

			Percentage	
Two-hour spells	Set	Approx. time after start (hrs.)	Average mark	Average time
Spell no. 2	5	1¾	99	103
Four-hour spells				
Spell no. 3	7	2	84	100
,, 4	9	2½	93	92
,, 5	6	1½	107	99
,, 6	5	1¼	97	105
,, 6	8	2	96	97
Eight-hour days				
Tues. a.m.	9	2¼	96	103
Thur. p.m.	3	1	95	95
Fri. p.m.	8	2	100	104
Sat. a.m.	4	1	102	94
,, p.m.	5	1¼	100	93

Table XIII. *Average marks and time for sets of* 10 *attempts before and after special efforts expressed as percentages of averages for the spells in which they occurred.*

A. *Marks.*

Four-hour spells	Approx. time after start hrs.	Average for last 10 before effort	Average for first 10 after effort	Average for second 10 after effort	Average for third 10 after effort	Introspection recording effort
Spell no. 2	2	83	92	69	104	Effort to wake up and do better
,, 3	1½	89	101	93	90	Pulled myself together
,, 3	2	87	107	92	95	Suggested confidence
,, 4	1	102	103	101	108	Tried extra hard; wanted 10 out of 10
,, 5	3¼	89	107	94	95	Suggested confidence
Eight-hour days						
Mon. a.m.	2	79	122	107	89	Determined effort
Fri. a.m.	1¼	95	93	105	105	Suggested confidence after nightmare feeling
Sat. p.m.	3¾	90	96	86	128	Special effort. Last set of experiment
Total average		89	103	94	102	

B. *Time.*

Four-hour spells	Approx. time after start hrs.	Average for last 10 before effort	Average for first 10 after effort	Average for second 10 after effort	Average for third 10 after effort	Introspection recording effort
Spell no. 2	2	99	134	132	87	See above
,, 3	1½	98	119	107	112	
,, 3	2	82	98	83	91	
,, 4	1	94	96	111	104	
,, 5	3¼	99	99	99	102	
Eight-hour days						
Mon. p.m.	2	104	97	92	93	
Fri. a.m.	1¼	102	104	89	89	
Sat. p.m.	3¾	102	101	114	119	
Total average		98	107	103	100	

Table XIV. *Average marks and time for sets of* 40 *'drowsy' attempts expressed as percentages of averages for spells in which they occurred.*

Two-hour spells	Set	Approx. time after start hrs.	Percentage: Average mark	Percentage: Average time
Spell no. 4	3	1¼	101	97
Four-hour spells				
Spell no. 2	5	1¼	98	106
,, 3	6	1¾	99	104
,, 3	11	3	90	88
,, 3	12	3¼	98	95
,, 3	13	3½	100	95
,, 4	8	2½	95	101
,, 4	14	3¾	99	94
,, 5	11	3	101	100
,, 6	4	1¼	107	103
,, 6	9	2½	98	92
Eight-hour days				
Mon. a.m.	11	3	99	101
,, a.m.	12	3¼	102	98
,, a.m.	13	3½	102	89
,, p.m.	5	1½	104	91
,, p.m.	8	2¼	91	97
,, p.m.	9	2½	96	101
Tues. a.m.	10	2½	93	99
,, p.m.	10	2½	100	108
,, p.m.	11	2¾	100	109
Wed. a.m.	6	1¾	103	98
,, p.m.	7	2	106	102
Thur. p.m.	6	1¾	100	92
Fri. a.m.	3	1	98	98
Sat. p.m.	9	2¼	92	98
Total average			99	98

was required in the latter experiment, or to the fact that while performing Guidit the subject had a very fair idea of the goodness and badness of her work. Both explanations are extremely tentative and based on insufficient data, but further investigation should help to solve the problem.

(iii) As well as the moods already described, many periods of a more passive kind of boredom were experienced in the course of the Guidit experiment. Many of these were accompanied by feelings of tiredness and of general drowsiness, and as such are detailed elsewhere in this report. Periods of this passive boredom, in which tiredness and drowsiness were not reported, are recorded above. (See Table XII.)

The average mark for these sets is 97 per cent. and the average time 99 per cent. of the averages for the spells.

(4) *Effort to improve.* The subject was trying to work accurately the whole time, but a special effort to improve was recorded eight times. (See Table XIII.)

(5) *General drowsiness.* This condition was frequently reported, it was usually described as 'Bored and sleepy.' Sets where it occurred unaccompanied by feelings of tiredness are detailed above. (See Table XIV.)

(*e*) *The effects of 'distractions.'*

Two kinds of distractions were observed: (i) when there was a noise outside, usually of a mowing machine, which lasted through a whole set, and (ii) distractions which lasted a very short time; there were also two cases of special interest distractions.

(i) Table XV. *Average marks and time for sets of* 40 *attempts during which the subject was partially distracted, expressed as percentages of the averages for the spells in which they occurred.*

Four-hour spells	Set	Approx. time after start hrs.	Percentage	
			Average mark	Average time
Spell no. 4	5	1¼	106	112
Eight-hour days				
Wed. a.m.	7	1¾	105	95
„ p.m.	4	1	107	110
„ p.m.	9	2½	96	103
Thur. p.m.	8	2¼	105	101
Total average			104	104

The results for these sets average the same as those for sets during which the subject sang to herself (see Table VIII). In both cases atten-

(ii) Table XVI. *Average marks for the* 10 *attempts before and after momentary distractions, expressed as percentages of averages for spells in which they occurred.*

Four-hour spells	Set	Approx. time after start hrs.	Average mark		Average time		Cause of distraction
			% for 10 before	% for 10 after	10 before	10 after	
Spell no. 3	8	2	95	99	91	97	Aeroplane outside
„ 5	9	2½	114	107	97	101	Someone coming into the room
Eight-hour days							
Mon. a.m.	6	1½	91	105	101	96	Someone coming into the room
Wed. a.m.	13	3½	99	115	103	102	Church bells outside
„ p.m.	8	2	86	111	93	109	Someone coming into the room
Total average			97	107	97	101	

tion was partly occupied by other things than the performance of the task and accuracy was slightly raised and speed slightly lessened. The distractions were disliked rather than liked.

The above distractions seemed to have a good effect on accuracy. It is not recorded whether the aeroplane and church bells were liked or not, but the interruptions of people coming into the room were very welcome indeed.

The first 'special interest' distraction was on the Monday afternoon of the week of eight-hour days. The subject was entertained by a drawing by an unknown hand on her scoring sheet, and marks and time for that set were 109 per cent. and 91 per cent. respectively of the averages for the spell. The other occasion was on the Saturday afternoon when the stopwatch refused to stop at the first press and simultaneously returned to zero at the second. This meant that it had to be observed very carefully at the end of each attempt and it created a new interest. Average marks rose from 99 per cent. for the previous set of 40 attempts to 110 per cent. for the following set, but the interest soon lost its novelty and average marks for the next set fell to 94 per cent. Average time for the three sets in question were 86, 90 and 90 per cent. of the average for the spell.

V. CONCLUSIONS.

The following general, but tentative, conclusions may be drawn:

1. That in all periods of one hour and upwards accuracy tended to decrease in the second as compared with the first half of the spells of work, the relative decrease in accuracy apparently increasing in spells of above one hour, but thereafter remaining fairly constant up to spells of eight hours. No longer spells than eight-hour periods were worked.

2. That there appeared to be a small involuntary increase of rate of work during the second as compared with the first halves of the spells irrespective of the length of the spell.

3. That in general accuracy improved slightly after prescribed pauses, more or less irrespective of their length, and seemed to decrease slightly after unexpected pauses enforced by some disturbing external circumstance.

4. 'Feelings of tiredness' were recorded in two-hour spells and above, and were almost consistently accompanied by a fall in accuracy (average fall about 10 per cent.), but by practically no change in the rate of work.

5. An attempt was made to record variations of 'conscious attitude' throughout the working spells, and it seems possible to make the following suggestions:

Cheerfulness and confidence are fluctuating attitudes, commonly marked at the beginning of a spell and thereafter varying irregularly. Whenever recorded they seem to mark a period of increased accuracy.

Special devices adopted to maintain cheerfulness may have a direct effect on the accuracy of the work. Singing, for instance, invariably appeared to improve accuracy and in general slightly to decrease rate of work.

Concentration on speed for short spells, adopted to maintain cheerfulness and confidence, generally resulted in increased accuracy as well as in increased rate.

Boredom was reported to fall into three types: (1) angry, (2) 'nightmare,' compulsive, (3) passive. During spells in which the first occurred there was an average 12 per cent. loss of accuracy, but no decrease of rate. The 'nightmare' type is where the operator has a hopeless feeling of being 'driven' to work by some force outside herself. Here again is loss of accuracy, but no decrease of rate. In passive boredom a much smaller loss of accuracy was observed and also a very slight decrease of rate.

General drowsiness seemed to produce a slight fall of accuracy and a slight increase of rate.

6. Distractions were recorded and were divided into two classes, *i.e.* outside distractions produced by external continuous noises, and momentary distractions of a special character, such as the coming of individuals into the experimental room. The first seemed slightly to increase accuracy and decrease rate of work: the second to increase accuracy more but to have little effect on the rate of work.

In conclusion I wish to express my thanks to Mr Bartlett for his help and advice.

(*Manuscript received* 21 *September*, 1927.)

THE CREATIVE ACTIVITY

INTROSPECTIVE EXPERIMENTS IN MUSICAL COMPOSITION

BY EVELYN BENHAM.

I. *Introduction* (pp. 59–60).
II. *Method* (pp. 60–61).
III. *Preliminary experiments* (pp. 61–62).
IV. *Mood sets* (pp. 62–63).
V. *Summary* (pp. 63–64).
VI. *Conclusions* (pp. 64–65).

I. INTRODUCTION.

THESE experiments in regard to the creative activity have been undertaken after a study of the habits of composers as noted in letters and biographies, and after a consideration of any comments made on the subject by psychologists interested in the problem specifically or as a general problem. The tendency in modern musical biography is to note what psychological attributes the habits of composers evince. This might be extended to a consideration of the extent to which these modes of procedure are manifested by different composers. It certainly leads logically to a consideration of the conditions which the composer finds most suitable, what occurs in his mind prior to writing, what happens *while* he is writing, the handling of his initial idea, and of his methods of writing in general. The study of records reveals undeniably the conscious recognition of preliminary activity, either of short or long duration. It also reveals the manifestation of kinaesthetic activity during the act of composing, the necessity for keen auditory imagery, and the questionable need of visual imagery. The study of articles in current journals reveals a general interest in the problems of the pre-existing whole, imagery with reference to art and literature, the activity of the subconscious, and the intellectual and emotional content of the inspirational experience. In regard to the subconscious, emphasis has been laid on the activity which must lead up to the 'flash' or 'spark' of inspiration. Dessoir, Prescott, Lowes, and Wallas have dealt with this subject, and Howes has commented on the musical aspect. Recent experiments on the creative process in art have been made by Paulsson.

The only possible scientific approach to the problem of creative activity in music has appeared to be the observation of imagery. The task is to ascertain just what auditory imagery passes through the composer's mind in a stated amount of time preceding writing, the evidence for visual imagery either of the notes or an extraneous nature, and any evidences of kinaesthetic or organic imagery. The nucleus is the initial imagery. The centre of problematical intensity is the exact point where the initial idea emerges. The focus then before starting these experiments was imagery, and the need of establishing some method for analysing that imagery.

Subsidiary results which might accrue are both musical and psychological. An awareness of conditions by the composer might lead to the possibility of procuring these conditions consciously. An awareness of his type of imagery might facilitate his production. Psychologically the study of the aesthetic experience as such might be aided by this particular focus.

II. METHOD.

In attempting these experiments, quiet surroundings were found. Pencil and ordinary manuscript paper with the treble clef sign were used. No bar lines, key or time signatures were indicated during the writing of the melodies.

The purpose in starting the experiments was to notice the general conditions facilitating writing, and to try to note the appearance of the initial idea. As they were continued, attention was concentrated on imagery of any kind connected with the initial idea. Any imagery observed in the minute preceding writing was noted for comparison. An effort was made to write with as little conscious effort as possible, so as to be unaware of the fundamental rhythm or key of the melodies, while avoiding pure automatism. As a result, each melody had to be analysed in order to make it understandable or usable.

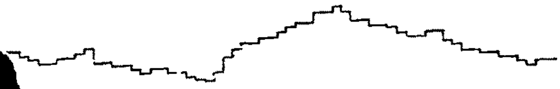
Notes were made during each experiment and at the conclusion of each one. It was found advisable to divide these notes into three sections. First, any observation about the preceding minute; second, notes on the writing of the idea; third, notes on the manipulation of the melodies after writing, *i.e.* addition of bar lines, key signatures, etc. Analysis often revealed unusual phrase balancing or time patterns of great variety. In the early experiments, any imagery was noted, with the emphasis on the auditory. Later experiments showed the need of individual analysis of visual, organic, kinaesthetic, in order to separate somewhat these types from their affiliation with the ordinary experience. An attempt was made to restrict the preceding minute to that time valuation, and to gauge roughly the time used for melody writing.

III. PRELIMINARY EXPERIMENTS.

Twelve melodies were written between September 14, and October 15, 1926. The observation in these early experiments was fragmentary. Visual imagery, it is stated, was not noticeable. "In one instance only is there no first impression of a nucleus of a few notes. I had no idea of the kind of melody that was coming. The writing of the first note—its place and kind—was definite, also the metronomic speed." "The melody does not come on any denoted instrument." "Kinaesthetic activity was not noticed." "The melodies are forgotten immediately after writing."

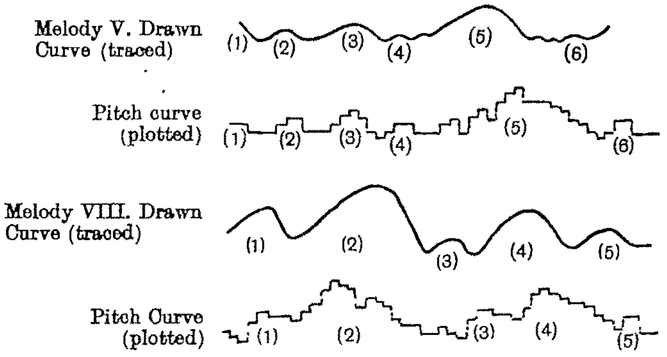
The second experiment consisted of six melodies written between October 21, and November 19, 1926. There was nothing exceptionally different in the writing or in the analysis, the latter being still vague. "It would seem—that the lack of recognition of tonality other than a note as tonal centre, facilitates the smooth writing under the specified conditions, as does the obscured indications of the rhythmical values."

The third experiment took place in another locality from January 30,
o February 19, 1927, covering ten melodies in all. The advance in analysis
curs in the recognition of "auditory imagery while writing, as though
ging." This 'singing' is apparently a combination of ill-defined
esthetic and auditory imagery—more auditory than kinaesthetic.
second innovation occurs in statements intimating a vague sort of
ic activity, "a muscular feeling of a rhythm rising and falling"—
writing seemed to afford rhythmical satisfaction." At the conclu-
the experiment, pitch curves were drawn of three or four of the
No. VI, where the 'muscular feeling' was particularly noted,
llows:



ingle space horizontally indicates a quarter tone,
. These curves suggested the problem of the
to the pitch curve, and whether there
c curve which was motor-muscular.

The fourth experiment had nine melodies from February 20, to March 16, 1927. An attempt was made to clarify the preliminary curve. A piece of plain paper was taken, the line drawn with the eyes shut, and the paper placed out of sight before the writing of the melody. In graphing the pitch curve of the melody for comparison, the horizontal single space equalled one eighth note, the vertical single space was the equivalent of two whole tones, accidentals not being indicated in this rough plotting. Little imagery of any kind seems to have been connected with the drawing of the curves. In No. IV, however, there is "auditory imagery at once when the preliminary curve is drawn—notes coming with the curve." The question immediately arises as to whether the rhythmic and auditory material exist in conjunction prior to writing. The notes regarding the melody writing show that memory of the general nature of the drawn curve is retained from the muscular action, but not the details. For comparison, the preliminary drawn curve was traced above the plotted pitch curve of the melody immediately following. Nos. V and VIII are reproduced, the various sections of the curves being numbered for comparison.



The most significant items in regard to the lines are first liminary drawn curve, and second, the way in which the of the drawn curve and pitch curve tally. This was noti degree in all of the melodies of this experiment.

IV. MOOD SETS.

A correlative series of experiments was January. 19, 1928, in an attempt to obse Certain mood sets were chosen, with as deliberately imposed would influence the mood and hence the melody as the expression of the mood. The following sets were planned:

(1)	Dejection, Lassitude.	Limp body; passive mind; drooping of arms, legs, head. Relaxed facial muscles.
(2)	Peace, Calmness.	Relaxed body; passive, but sustained, mental attitude; drooping, no tension.
(3)	Joy, Elation.	Straight back, head tilted back; eyes lifted; note mouth muscles; hands clasped; note shoulders.
(4)	Energy, Decisiveness.	Purposed tension of body; hands slightly clenched; head bent forward with body; mind concentrated.
(5)	Sorrow, Suffering.	Head on hands; elbows on knees; tension of mind and body.
(6)	Super-restraint.	Cf. (4). Tension of mind and body, inhibiting action, but with similar bodily appearance.

The physiological details were amplified when the experiments were made, and notes made as to the maintenance of the set, and the mood. Nos. I and VI were tried twice, No. II three times for better musical results. In every instance, the physiological set as planned was achieved in the preceding minute with a mood of greater intensity than planned. After writing, notes were made regarding the maintenance of mood and set during the writing, and further notes were made regarding manipulation of the melody, and its value with reference to the mood. It was reported that the mood set was maintained in every writing, although the physiological conditions necessarily had to be altered. In every instance the melody was a consistent expression of the mood desired and these expressions were clearly differentiated. The precautions used in writing the previous experimental melodies were maintained here, and notes made on any awareness or lack of it while writing. The most successful was No. V, where the mood imposed was very unlike the preceding state of mind, and where the musical results were found to tally in a manner not realized while writing them.

The possible conclusions are (i) that certain motor adjustments can be deliberately imposed, (ii) that these adjustments can be sustained at will for a certain period, (iii) that these adjustments produce simultaneously a mood correspondence which may have been wholly lacking at the outset, (iv) that this set super-imposed affects the content of the output—in this case, the musical expression in a melody.

V. SUMMARY.

The average rate of speed in writing the melodies was 60–70 seconds, the average length of melody being nine measures. The tonality was consistently minor, *d* and *e* predominating. Phrasing and time patterns were irregular. The average time was 4/4, although there was frequent use of 5/4, 6/4, and 7/4. One third of the melodies were satisfactory musically, and have since been used. The melodies were evenly divided as to tempo. The reports on visual and kinaesthetic imagery were not definite. Auditory imagery was reported in the 'singing.' The curves presented a new problem in imagery. To what extent is auditory imagery a fluctuating phenomenon? To what extent is combined kinaesthetic and auditory imagery essential? What suggestive value is existent in the curve data?

In the mood set experiments, the average rate was 60–70 seconds where noted, the average length of melody being twelve measures. There were six melodies of major tonality to four of minor. The average time was 4/4, except for No. VI, 2, which was 6/4–4/4 alternately, and No. V which was patterned 5/4–3/4–3/4, as analysed after writing. No. V was particularly notable in the maintenance of a mood not at all present before the experiment, and in the effect of that mood upon the music.

VI. CONCLUSIONS.

These investigations indicate that definite evidence can be obtained regarding the preliminary activity in composing. There is evidence of motor sensations which may be allied with the rhythmical aspects of the music. Auditory imagery is apparently present, for which trained observation is necessary. It is a question whether this auditory imagery is present previously as a major item, or whether the observation of it is the focussing upon one aspect of an imagery which is a synthesis. Or it may be that the various kinds of imagery can be more easily observed in the preliminary period, and the closer synthesis lies in the writing of the initial idea. Certainly the mood set, to be effective, should be established in the preliminary period.

In regard to the initial idea, the auditory imagery is strongest at the emergence of the idea, strengthening the thesis that that point is the centre of problematical intensity. Observation should be made regarding kinaesthetic and other imagery, to see whether they also are strongest at that point. It would appear that the control of mood set is possible and that a mood pre-established can be maintained for a stated time as desired.

Ultimate value might therefore seem to be possible for the musician in the control of subconscious activity, of mood sets, and in the development of an awareness of mood, and of imagery. This activity would sharpen his musical ideas. For aesthetics, there would seem to be a connection between mood sets and musical appreciation, and the need of observation of imagery.

In these experiments, the intention is to clear up correlative experiments, such as experimenting with visual imagery, before analysing the auditory as such. After the visual and the auditory, an attempt will be made to cover the various aspects of imagery in a single series. Then should follow the objective attack, dealing both with the phases of imagery, and the grouped series, in order to establish the validity of the introspection.

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(*Manuscript received June* 17, 1929.)

RESULTS FROM SOME NEW TESTS OF PRACTICAL ABILITIES

By W. VICKERS and V. H. HOSKIN.

(*From the Department of Education, University of Sheffield.*)

I. *Introduction* (p. 66).
II. (*a*) *Description of tests* (pp. 67–72).
(*b*) *Children and workpeople tested* (pp. 72–73).
III. *Results and conclusions* (pp. 73–80).
IV. *Conclusions summarized* (pp. 80–81).

I. INTRODUCTION.

ALL tests of practical ability involve in varying degrees at least two types of activity, namely, intelligence and bodily skill. At one extreme there is such a test as the Picture Completion Test, where meanings must be arrived at through pictures with or without the use of words, and where the reasoning is based upon these meanings so arrived at. The manual skill needed for placing the pictures in the recesses is (from the standpoint of its grade or the time it occupies) both small and unimportant. The other extreme—where the intelligence needed for grasping the problem presented by the test is of a low order, and yet where the bodily dexterity demanded is of a high order—is represented by such a test as the "Moving Line" Test to be described.

The tests of practical ability here to be considered were evolved originally in order to carry out vocational guidance experiments in two manual occupations, namely, machine file-cutting (a moderately skilled occupation) and machine tool-making (a highly skilled occupation). These occupations were carefully studied and analyses made of their psychological requirements. Tests were then devised in accordance with these analyses. Certain of the tests were intended to measure specific abilities; others had as their object the measurement of a postulated more general practical ability.

In order to standardize the test material and procedure the series was tried out on 115 thirteen-year-old school children. When the tests had been made sufficiently reliable they were given to workpeople.

II (*a*). DESCRIPTION OF TESTS.

Obstacle Test. The problem in the Obstacle Test was easy to grasp, but the solution involved a moderately high level of reasoning. If (as James and Professor McDougall argue) the essential feature of reasoning is the sagacious selection of relevant provisional solutions to problems, then, whereas the usual type of reasoning test gives scope for the exercise of reasoning on the imaginative plane, the Obstacle Test gave it scope on the practical plane. The test involved this sagacious selection of cogent hypotheses but on the level of practical trial and error. Delicacy of touch, dexterity in manœuvring materials into position, clean cut movements and persistence in the face of repeated failures, were further minor factors present in this form of the test, although they can be made to play a large part in other forms.

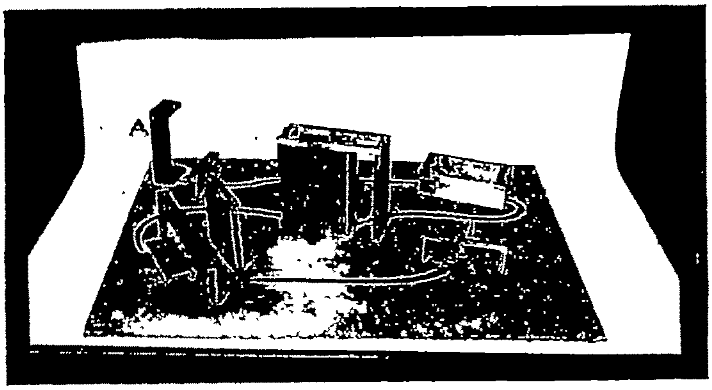

Fig. 1.

The Obstacle Test (see Fig. 1) consisted of a board on which was mounted a series of obstacles graded in difficulty. Through these obstacles the subject was required to manœuvre the wooden object (*A*). The subject was told: "This is an obstacle race, and this is the piece which has to be taken through them. It first goes through this one and then this, etc." (The experimenter indicated the order of the obstacles and the entrances and exits. A line on the board served to remind the subject of the order in which the obstacles were to be taken.) The experimenter then continued: "It does not matter how you take the piece through so long as you do not use force. As a matter of fact, when you discover the correct way the piece will slip through quite easily. Go through the obstacles as quickly as you can. Now begin."

The times taken to overcome each obstacle were noted. The second and third obstacles in the figure were regarded as one obstacle. A maximum time limit was set to each piece for practical reasons. These limits corresponded for each obstacle to the points 1·5 σ of a group of thirteen-year-olds. They were as follows: (1) 60, (2) 70, (3) 120, (4) 100, (5) 60—all expressed in seconds. The shortest total time recorded for school children (120 cases, boys and girls, 13½ average age) was 54 seconds, and the longest time 396 seconds. The time for each obstacle was converted into a weighted score. The maximum scores allotted to the individual obstacles were made inversely proportional to the percentage of the failures in the above group at each obstacle at the end of 20 seconds. These percentages were: (1) 15, (2) 19, (3) 90, (4) 86, (5) 90. Tables, for converting to test scores the time scores of each subject for each obstacle, were made by multiplying the times, expressed in terms of the standard deviation of the above group of children, by the appropriate weight.

The 'reliability' (or, as it is called later, the 'consistency') coefficient obtained from this group by this test, was 0·80 ± 0·04.

If this type of test is found to be applicable to other industrial occupations, then it seems worthy of an effort at systematic standardization for the purposes of vocational guidance. It is being further exploited.

Balancing Test. The materials required for this test are a pointed ink eraser to act as a pivot, a Gestetner "black metal plate," and some weights suitable for attaching to the edges of the plate.

The subject was required to balance the plate horizontally on the pivot by moving the plate by means of the finger tips which were placed under the plate. In order that the plate might not be balanced by an estimation of the geometrical centre, two weights were attached to the edges. This had the effect of moving the balance point from the geometrical centre. Variations of the test have been made, for the purpose of practice and for the purpose of obtaining a reliability coefficient, by adopting the simple expedient of turning the plate round or upside down. A practice and four trials in all were given.

On the basis of experience gained by testing the above thirteen-year old group, a time limit of 90 seconds was set for each trial, and a scoring table was drawn up. This table expressed time scores in terms of the standard deviation of the group.

A 'reliability' (or 'consistency') coefficient of 0·65 ± 0·05 was obtained on testing the above group.

It was not intended that this test should measure any specific ability, since the movements it demands are in no way comparable to those found either in Engineering or School Handwork. This test probably evaluated certain temperamental traits such as care and caution, besides being a test of practical ability. The subjects soon discovered that the best way of balancing the plate was by moving it quite a small distance at a time, and after each move to make a 'trial balance.' This served as a guide to the direction of the next small movement to be made. Thus care played an important part in the degree of the subject's success.

Inverse Formboard. A modification of the usual simple formboard was adopted. The chief difference lay in the features that the forms were mounted on larger discs and they were the mirror images of the recesses. Thus each form when viewed by the subject appeared as the mirror image of the recess into which it was to fit, but when the form was inverted ready to be placed in the recess, the disc mount hid the form from view. Thus any trial placement had to be made by actual movement, it being impossible to effect the trial by simple direct visual comparison.

The number of moves made by the subject were recorded in addition to the time taken in completing the test. Time scores and move scores were again transmuted into composite standard scores of the thirteen-year old group.

Moving Line Test. It appeared desirable to obtain a test which would include varied sweeping wrist and short arm movements. Tests of the formboard and pegboard type allow of no varied movements, but require a number of movements of approximately the same description. These are, moreover, jerky and straight as opposed to the natural flowing movements of all skilled performances. Hence accurate sweeping movements which required also a steadiness of hand have been embodied in a 'moving line' test. The aid of vision to a test of this description is apparent, but the evidence of introspective data points also to the material help of kinaesthetic imagery.

In the test the subject was required to trace a wavy copper line which appeared on a revolving drum. The tracing was done through a fixed aperture by means of a metallic pencil. So long as the subject was able to trace the line perfectly, electrical contact was maintained and the movement of a chronoscope was suspended. But as soon as the subject's pencil left the line electrical contact was broken and the clock began to register. Thus whilst the subject kept to the line the clock did not register, but the oftener he left the tracing the longer was the time recorded. The final time reading of the chronoscope gave the score directly. The lower the time score, of course, the better the test score.

A copper wire of square cross section was soldered on to the curved surface of an ordinary smoke drum and the space between the lines of wire was filled with ebonite to the level of the copper. When finally turned and polished, there was displayed on a smooth surface of ebonite a wavy copper line.

The design of the copper line had been previously determined by experiments on school children, using a line drawn on a paper which covered the drum. The degree of difficulty of the pattern, the speed of the drum, the width of the line, and the dimensions of the aperture, were in this way decided upon before the drum was actually made. Two types of lines were used (Moving Line A and Moving Line B). Moving Line A consisted of long waves. This test was one which estimated skill in the movement of the wrist and forearm. The Moving Line B, however, consisted of short curves of small amplitude. The drum, moreover, revolved at a lower speed than in the former test, and so made the test longer. The movements necessary to follow the line were simple enough, but it was necessary to have no straying of attention.

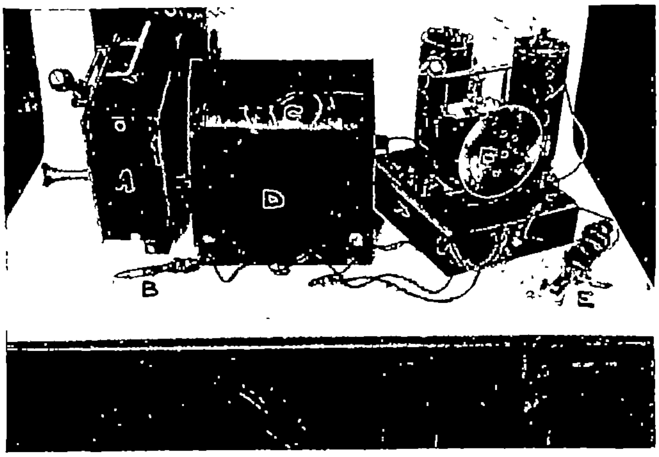

Fig. 2.

Fig. 2 shows the layout of the apparatus. *D* was a screen with an aperture through which a part of the drum *C* could be seen. *A* was the clockwork mechanism which drove the drum. The pencil which the subject held is indicated by *B*. It was connected to the D'Arsonval chronoscope *F* by an insulated cable. An auxiliary control of *F* was possible with a key *E*.

The test started with the chronoscope's action suspended because the key *E* was caused to make contact. Afterwards the key, which was normally open, was dispensed with until the end. The standardized instructions were: "Here is a revolving drum which has on it a wavy copper line. In a moment take this pencil and trace the line as carefully as possible. The method of holding the pencil is this." (The experimenter demonstrated. The pencil was so held that the distance between the fingers and the contact point was constant for all. This was done by having a small groove in the pencil.) The experimenter then continued, whilst demonstrating: "Now place the pencil on the black part of the drum, pressing with medium strength, and wait for the line to come into view. As soon as the line appears, transfer the pencil on to it from the black, and then try to keep on the line all the time, taking care not to take the pencil off the drum."

When next the line appeared the subject started the test. The line was so designed that the first part was nearly straight in order to give the experimenter the chance of starting the clock as soon as it was seen that the subject had begun the tracing. Gradually the curve became more difficult, but it finished as it began in a manner easy to trace. Finishing thus had two advantages, one being that it was easy to stop the chronoscope, and the other that the attitude of mind left with the subject was that, at any rate, he was not a complete failure.

Each line was traced three times, the first one counting as a practice. When tried out on a class of 36 schoolboys (mean age 13½) the correlation between the second and third attempts was found to be + 0·88 ± 0·06. Thus the test is consistent.

Area Sorting. This was devised to test the specific ability necessary to a certain extent in machine file-cutting. A pack of cards was required to be sorted according to the areas of the rectangles described on them. The consistency coefficient when 30 cases were tested was found to be 0·84 ± 0·07.

Aiming Test. This was an adaptation of the test described by Whipple. Instead of a series of crosses a zigzag line was substituted. The subject aimed at each corner of the diagram zigzag to the beat of a metronome. The zigzag was of such a shape that the subject could see, when hitting one target, the position of the next target. The consistency coefficient was 0·71 ± 0·09.

Substitution Test. This was an adaptation of the Woodworth Wells

Substitution Test. Cards bearing geometrical figures had to be sorted into compartments. This again was a test for a specific ability useful in file-cutting.

Abstraction. It was found in preliminary work that the usual types of intelligence tests involving, as they do, responses usually in words, are very unsatisfactory when applied to adult subjects. An intelligence test was therefore designed requiring card sorting as the only response. The subject had to ascertain the several similarities common to geometrical and other drawings on a card, and place it in one of many compartments which bore specimen cards on which were drawn some more figures having common features. The cards were to be sorted into the appropriate compartments. The consistency coefficient was 0·69 ± 0·05.

Judged Reaction Test. The subject was required to stop an ordinary D'Arsonval Electric Chronoscope when the finger of the latter came opposite to a definite mark on its face. This was effected by a hand switch. The errors made in 40 attempts to stop the clock at the required mark were noted, and the mean of these transmuted into standard scores.

The test was devised to measure the accuracy with which a subject could stop a moving object at a certain place.

A consistency coefficient of 0·70 ± 0·09 was obtained.

A Slide Rest Test. A somewhat similar type to that used by Mr Max Tagg was adopted[1]. A practice was allowed.

The consistency coefficients were found to be: for accuracy 0·92 ± 0·02; for time 0·98 ± 0·01.

Cube Imitation, Picture Completion Test No. 2, and Cube Construction Tests. These were given as described by Miss Frances Gaw in *Report No.* 31 of the Industrial Fatigue Research Board.

II (*b*). CHILDREN AND WORKPEOPLE TESTED.

Besides the above-mentioned group of school children, there were tested six groups of workpeople in three different works in Sheffield[2].

Conferences were held with these groups when the vocational guidance aspect of the present investigation was explained and their approval gained. Some details of the groups are as shown in Table I.

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[1] *J. Nat. Inst. Indust. Psych.* 1925, II, 321.

[2] The writers are under a debt of gratitude to the firms of Messrs Samuel Osborn and Co. Ltd., Messrs Thos. Firth and Sons, Ltd., and Messrs Cammell, Laird and Co. Ltd. for placing facilities at their disposal and for giving whole-hearted help.

Table I. *Details of groups tested.*

Group no.	Sex	Age extremes	Mean age	Occupation	No. in group
I	Males	15–20	16	Machine file-cutters	15
II	Females	16–30	20	Ditto	19
III	Males	23–50	38	Ditto	15
IV	Females	16–32	23	Ditto	16
V	Males	14–42	21	Machine tool-makers	34
VI	Males	18–52	28	Ditto	30

III. RESULTS AND CONCLUSIONS.

1. The amount of reliance that can be placed on the results needs first to be estimated. The estimation must be fourfold. Firstly, the 'stability' of the results has to be gauged. In other words, the question has to be asked: "Would similar results be obtained if other groups of subjects were tested?" The fairness and size of the sampling are here the main considerations. Secondly, there must be known what is too vaguely called the 'reliability' of the tests. This characteristic, better termed 'consistency,' can be estimated by the correlation coefficients obtained when two forms of the same test are given to the same group of persons. Thirdly, the motivation and the attitude of the subjects affect the reliance placed upon the results. And lastly, it must be known whether the tests really measure what they set out to measure. These four points will be discussed *seriatim*.

(i) As will be seen from Table I, the numbers in the Groups I to IV were small, but they were as large as could be obtained from under the charge of any one foreman. Four groups were tested in order to compensate partly for this defect. Groups V and VI were of more adequate size. With the exception of Group IV, in which some women who were suspicious of the tests were not included, whole groups under one foreman were tested, and so the selection was due only to industrial factors.

(ii) The consistency of the tests may be judged from the data given in 2 (*a*) above, where each test is described. The coefficients vary from 0·98 to 0·65 and are usually greater than 0·8, which may be counted high.

(iii) Considerable time and trouble were spent in obtaining not only the goodwill, but the enthusiastic co-operation of the workpeople, a large number of whom were piece-workers, and who gave their time willingly.

(iv) The two sets of tests (see below) were designed to estimate file-cutting skill and machine tool-cutting skill respectively. Their validity may therefore be estimated by finding the correlation of the tests with the foremen's estimates. These coefficients are given in Tables II and III. It will be seen that there is considerable discrepancy between the results obtained from the various groups, but that certain tests have high validity in certain groups.

Table II. *Correlations between the tests and foremen's estimates.*

(File-cutting.)

Test	Group I		Group II		Group III		Group IV	
	r	P.E.	*r*	P.E.	*r*	P.E.	*r*	P.E.
Cube construction	·77	·11	·41	·14	−·02	·17	·26	·16
Aiming	·72	·12	·32	·15	−·02	·17	·19	·16
Abstraction	·62	·14	·38	·14	−·40	·14	·07	·17
Picture completion	·56	·14	·29	·15	−·42	·14	−·21	·16
Substitution	·40	·16	·52	·13	−·35	·15	−·34	·15
Cube imitation	·37	·16	·62	·12	−·51	·13	−·44	·15
Area sorting	·26	·17	·48	·14	·00	·17	—	—

Table III. *Correlations between the tests and foremen's estimates.*

(Machine tool-making.)

Test	Group V		Group VI	
	r	P.E.	*r*	P.E.
Moving line A	·71	·06	−·35	·11
Cube construction	·62	·07	·01	·12
Moving line B	·58	·08	−·06	·12
Inverse formboard	·49	·09	·13	·12
Balancing	·48	·09	·21	·12
Slide rest	·35	·10	·19	·12
Obstacle	·33	·10	·02	·12

2. It was very disconcerting, and later, illuminating, to find that these consistent and otherwise reliable tests seemed to vary in validity from group to group. Especially was it interesting to obtain such high negative correlations. These seemingly contradictory results could be caused by the combination of at least two factors. Firstly, the less the spread of ability within the group (given a constant individual variability) the less will be the correlation coefficients. The coefficients of variability showed this factor to be unimportant in Groups I to IV, but of some importance in lowering the correlations of Group V nearer to the size of those of Group VI.

Secondly, there can be the complicating factor of age. The groups varied, it has been noticed, very much in their age composition as well as in sex. Now all workmen begin their careers at approximately the same age; therefore age and experience are here nearly synonymous terms. The relationship between age and skill in file-cutting or machine tool-making was investigated. The correlations between age or experience and foremen's estimate of industrial skill were:

Group I	·98 ± ·05	Group IV	·48 ± ·13
Group II	·24 ± ·07	Group V	·83 ± ·04
Group III	·78 ± ·07	Group VI	·47 ± ·10

The regression line in each case is nearly rectilinear. In Fig. 3 are plotted against age of workmen, the foremen's estimates of workmen's skill in their work. From the above correlation coefficients and the curves of Fig. 3, it will be seen that, in general terms, the more experienced the workman (at any rate, up to the age of 50), the greater his skill in his occupation.

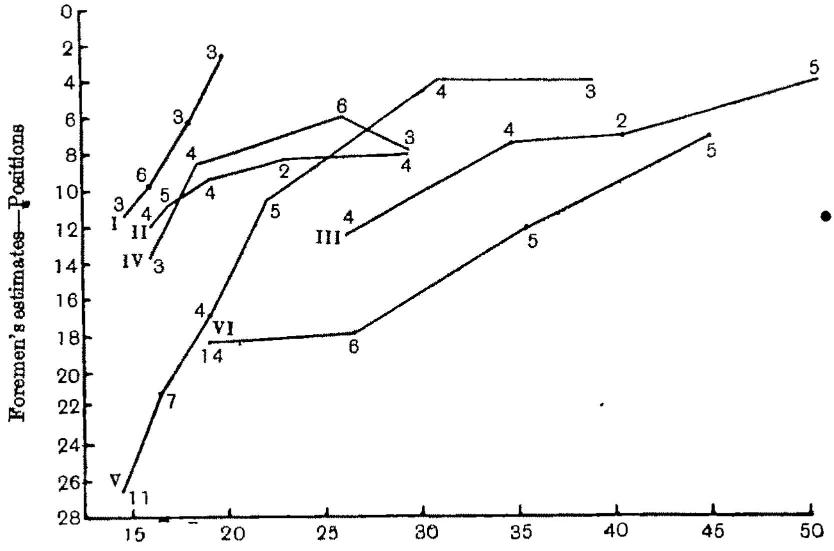


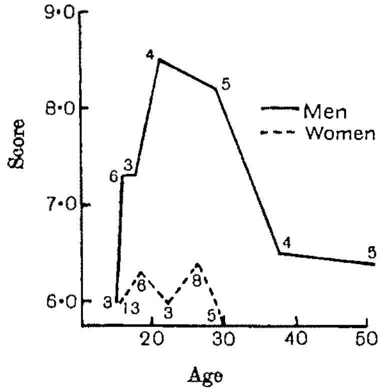
Fig. 3. Variation of age and skill in occupation.

(The small numbers on the curves give the number of cases that have been averaged to fix the points. The roman numerals indicate the group.)

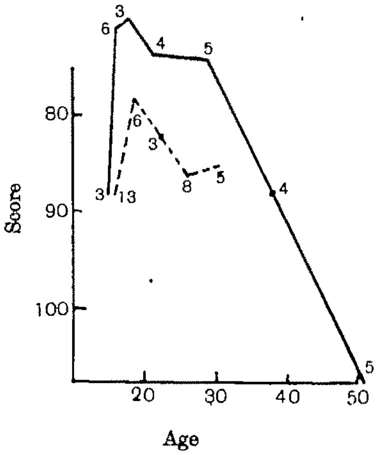
There remains, then, the question of how ability to perform the tests [illegible]d with experience. The graphs of Fig. 4 plot age of workmen in all [illegible] against ability in each test separately. It will be seen that in [illegible]se the curve is of the same type. The test abilities increase [illegible]up to about 25 to 30, and then decrease again to the age of 50. [illegible]ss of the cases makes this conclusion tentative only, but it is [illegible]gthened by the fact that the phenomenon occurs in every one of the tests. It is further strengthened by the fact that each group, from no matter which of the two occupations tested, shows the same characteristic. Fig. 4 (*b*), Curve 11, illustrates this in the case of the Cube Construction Test.

The seemingly varying validity of the tests as witnessed by the coefficients in Tables II and III may now largely be explained by the

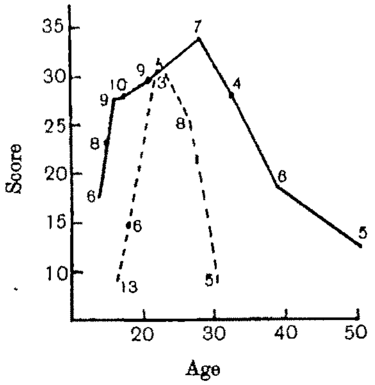
Curve 1. Cube Imitation.



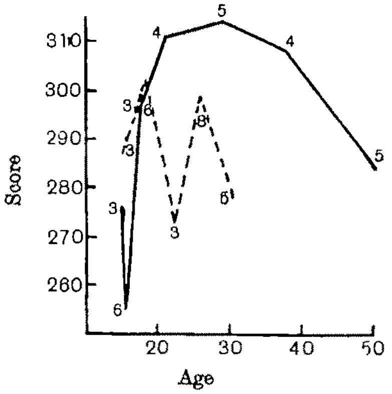
Curve 2. Substitution.



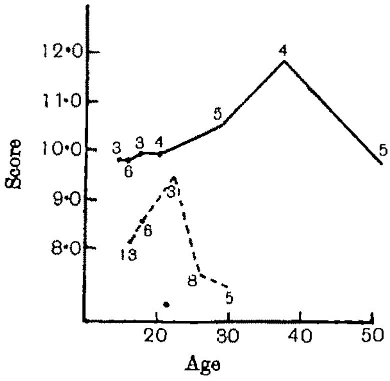
Curve 3. Abstraction.



Curve 4. Aiming.



Curve 5. Judged Reaction.



Curve 6. Picture Completion.

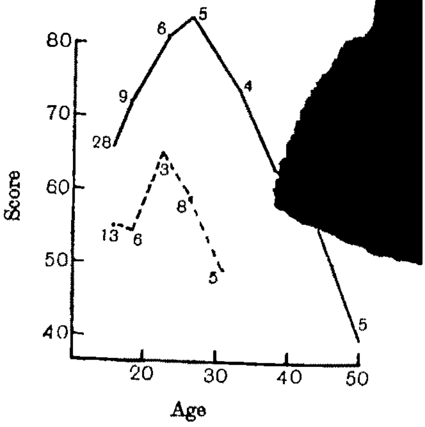
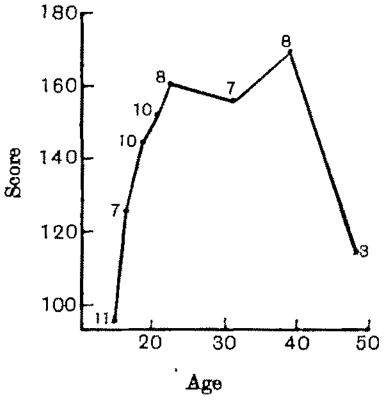
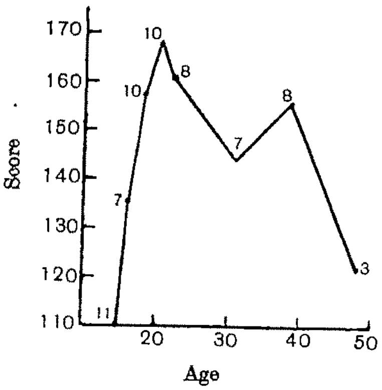


Fig. 4*a*. Variation of test ability with age.
(The small numbers on the curves give the number of cases that have been averaged to fix the points.)

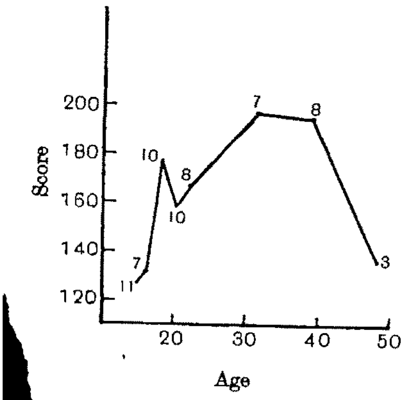
Curve 7. Obstacle.



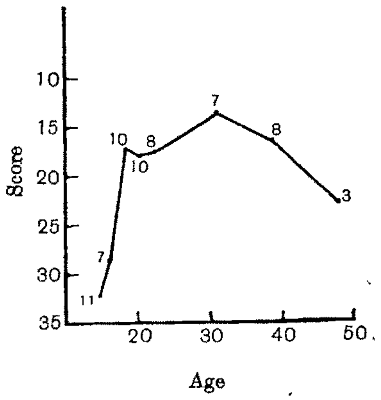
Curve 8. Inverse Formboard.



Curve 9. Balancing.



Curve 10. Moving Line A.



Curve 11. Cube Construction.

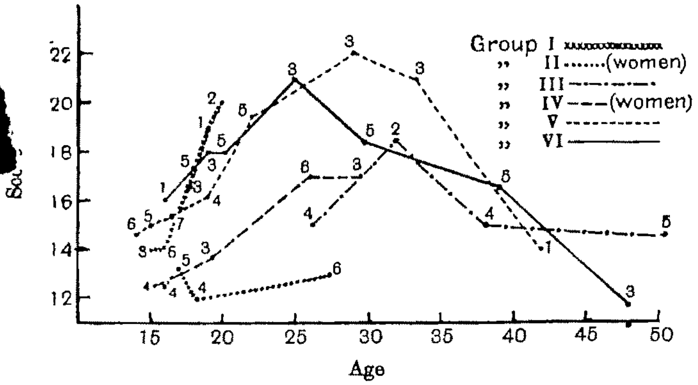


Fig. 4 b.

joint effect of (i) the curvilinear relationship shown to exist between age and ability to do the tests; (ii) the rectilinear relationship shown to exist between experience and skill in industrial work; (iii) the variability of the groups and (iv) the facts of the age composition of the six groups. Thus Group I had high positive coefficients, because all members of the group were below 25 years of age; Group II had lower positive coefficients because the group contained four persons over 25 years of age, though the effect of this was lessened by there being a low correlation between age and industrial skill in this group; the high negative coefficients of Group III were caused by there being all but one person *above* 25 years of age; in Group IV the ages were evenly distributed with their mean at 23, and consequently the mean correlation coefficient was just positive; only six were over 25 in Group V, and the average coefficient was moderately high; whereas the coefficients of Group VI were on the average nearly zero since the group was nearly evenly distributed on either side of the age 25, and the variability of the group was less than in Group V. Thus the results from each group were actually in agreement.

3. If the above conclusions with regard to test ability, age and industrial skill, be confirmed for other groups and occupations, it would have wide theoretical and practical applications.

(i) It might well be that the tests do really measure practical capacities, in which case the conclusion might be stated in the terms that whereas practical *skill* improves continuously with experience, practical *capacity* reaches a maximum and then declines. Age is seen acting as an interference factor[1].

(ii) This situation is analogous to the generally accepted fact th
the higher intellectual skills increase continuously with experience, b
intellectual capacity reaches its maximum fairly early in life and t
declines.

(iii) The conclusion is at variance with the generally accepte
well-supported one, that intelligence in average persons reac
height not later than 16. The usual intelligence tests are in th
verbal, and perhaps the predominant factor measured is facility a
accuracy in dealing with meanings *via* words. Now none of the intelligence tests used in this investigation (viz. Cube Construction, Abstraction, Picture Completion, Inverse Formboard and Obstacle) needed for their correct practical responses—responses needing only a low degree of skill—the facile or accurate use of words. They needed merely com-

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[1] Cf. J. R. Thompson, this *Journal*, 1919, x, 81–100.

prehension of meanings. The results show in these cases a progressive increase in intelligence up to 25 or 30 years of age and then a decline.

(iv) The curves give the suggestion that the maximum of intelligence might be reached somewhat before that of practical ability.

(v) A technical application of the conclusion lies in the construction of trade tests. The usual method of trying out trade test batteries on poor, medium and excellent journeymen needs to be modified. Such a selection of men needs the qualification that the journeymen should be under 25 years of age.

4. It will be noticed that the two groups of women score considerably worse than the men in the tests. Groups I (men) and II (women) worked under the same foreman, and under the same conditions. The same remark applies to Groups III (men) and IV (women). This result which, owing to paucity in numbers, is merely an indication, is in keeping with other measurements that have been made of a sex difference in practical ability.

5. To discover whether there was any group factor of practical ability manifested in the tests, the results from Group V were further treated statistically. Professor Spearman's tetrad difference method was taken as valid. The partial inter-correlation coefficients of the tests, keeping age constant, are given in Table IV. This procedure of keeping ge constant was judged fair since the group had five members just over 5 and a sixth 42 years old, and the relationship between test ability d age passed Blakeman's criterion of linearity.

Table IV. *Inter-correlations of Tests (with age constant).*

	1	2	3	4	5	6	7	8	9
varie completion	—	·50	·31	·62	·27	·50	·47	·15	·25
groups line B	·50	—	·32	·25	·78	·23	·28	·20	·25
	·31	·32	—	·51	·20	·35	·42	·34	·24
every ca uction	·62	·25	·51	—	·50	·13	·16	·31	·19
A	·27	·78	·20	·50	—	·51	·23	·14	·00
6. Balancing	·50	·23	·35	·13	·51	—	·40	·08	·14
7. Abstraction	·47	·28	·42	·16	·23	·40	—	·48	–·22
8. Inverse formboard	·15	·20	·34	·31	·14	·08	·48	—	·12
9. Slide rest	·25	·25	·24	·19	·00	·14	–·22	·12	—

The 378 tetrad differences of this Table give a histogram (Fig. 5) which is not normal. The theoretical probable error due to the operation of chance sampling errors, utilizing Spearman's formula[1], is 0·0558, whereas the actual median difference was 0·0519. The shaded portion indicates those portions of the obtained histogram which are significantly different from the theoretical values. There were 24 of these, that is, there were 24 tetrad differences caused by group factors. The significantly great tetrad differences were fairly evenly spread throughout the square table, except that the Inverse Formboard and the Slide Rest Tests were involved in very few, and the Moving Line A and B (which, of course, have much in common), Picture Completion, and Abstraction Tests were involved the most frequently. The actual group factors at work were not clear, although by the method of partial correlation (*g* constant) the indications were that the manipulative skill tests went together and that the rapid shifting of attention involved in the Slide Rest Test was an interference factor in those tests which needed steady concentration of attention.

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[1] Spearman, *Abilities of Man*, Appendix, p. xi, formula 16 A.

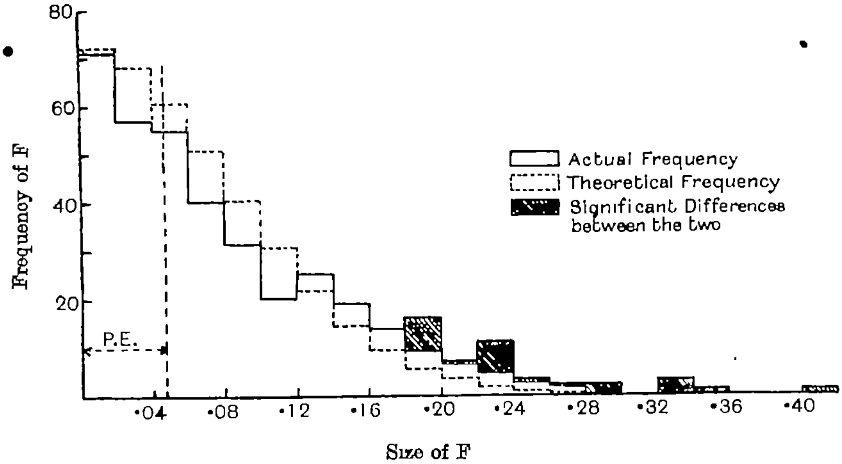

Fig. 5. Theoretical and Actual Histograms of F's.

The amount of *g* in some of the tests of skill was surprisingly large. For example, in the Obstacle Test there was 0·69, in Balancing 0·56, and in Moving Line B 0·52, whereas in the tests involving a negligible amount of manual skill there was only a comparable amount, *e.g.* Picture Completion 0·68, Abstraction 0·56[1].

IV. Conclusions summarized.

1. Tests of practical abilities have been devised giving consistent results.

2. Practical ability increases up to the age of 25 to 30 and then decreases, whereas practical skill (at least in file-cutting and machine tool-making) increases progressively up to 50 years of age.

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[1] Calculated by formula 20, Appendix, p. xvi of Spearman, *Abilities of Man*.

3. Intellectual ability, when tested in a practical situation, also increases to a maximum at 25 years and then decreases.

4. Age is thus shown to act as an interference factor.

5. The women scored less than the men in all the tests.

6. One or more group factors are shown to be present.

(*Manuscript received* 20 *July*, 1928.)

A SPEED-ACCURACY COMPETITION

By W. J. STAINER.

1. Purpose and plan of experiment.

A former article[1] dealt with variations of rate of work in schools under ordinary conditions. The present experiment was designed to secure definite improvement in the speed and accuracy of the mechanical processes involved in arithmetic, and in the retention and reproduction of English words.

Whereas the tests described earlier were intended to measure, under normal methods of instruction and stimulus, variations in rate of work throughout a school day or term, those described in this article were designed to ascertain by statistical methods what amount of improvement could be made by the systematic use of definite tests, accompanied by various carefully selected forms of stimuli.

The arithmetical tests were made much more complicated and difficult than in the former enquiry, and others (spelling) were added, the time allotted was much increased, and the exercises were followed by time devoted to corrections, to suggestions as to methods of calculating, and to noticeable features in the spelling of certain words. In short, every effort was made to show to the full the effects of practice and adaptation exerted through a whole year.

One period per month, on a fixed day (*e.g.* the fourth Thursday) at a fixed time, was assigned to each subject, 10 minutes being allotted to the actual working of the papers, the remainder of the period to corrections.

The exact timing of the tests was arranged as in previous experiments.

The Forms were thrown together for purposes of competition, in groups of Forms, *e.g.* Sixths, Fifths, etc.

Samples of the arithmetical exercises are:

Add	6786	9473		
	9378	5947	etc.	
				4 points each
Subtract	6347	8234		
	3986	4795	etc.	
				4 points each

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[1] This *Journal*, xix, 439–451.

Multiply $\begin{array}{r} 5684 \\ 9 \\ \hline \\ \hline \end{array} \qquad \begin{array}{r} 9538 \\ 8 \\ \hline \\ \hline \end{array}$ etc. 4 points each

Write down Quotient and Remainder:
68 ÷ 9 86 ÷ 17 etc. 2 points each

These were repeated in series, until the maximum points available amounted to between 500 and 600. No pupil, at any one of the tests, attained to the maximum points.

For the subject of spelling, lists of words frequently misspelt in the school were compiled. Some were given correctly, some incorrectly (in the common misspelling). The instructions were:

"Write (✓) against all words which are correctly spelt. Against all that are wrongly spelt put a cross (X), and then write the word correctly." One point was awarded to each correct effort.

Different numbers of words were used for different groups of Forms, and many words were used, month after month, until they were correctly spelt by nearly all the pupils.

'Speed-Accuracy Indexes' were assigned to each pupil and to each Form in arithmetic and in spelling, and in the two combined the results of each test were posted in each Form-room on a sheet large enough to contain the reports for one year. Each pupil was therefore enabled to check not only his own progress, but also the progress of his Form.

The results were also cast into 'House' order, so that each pupil knew not only his own personal 'Speed-Accuracy Index' but was also informed as to the extent to which he was helping to obtain that of his Form, and of his House.

In addition to the personal stimulus afforded by ordinary types of competition, and to those due to the usual play of the 'team' spirit, it was thought well to add other incentives, intangible yet appreciable.

To each Form at the head of its Group on any occasion, the title of 'The Champion Form' of the Group was awarded, and any Form which improved its position in its Group was given an additional 'Games period.'

A silver cup (the Speed-Accuracy House Cup), held for one year, was assigned to the House obtaining the highest index during the year.

It was distinctly understood that, other than by means of the competition, no unusual attempt was to be made either to teach spelling in an explicit manner, or to secure accuracy in ciphering, and no interference with the ordinary routine was to occur.

There is considerable reason to believe that the psychological effect of setting up an ideal of accuracy and of the regular and systematic return to the tests, coupled with the attention and interest aroused and the incentives offered to progress in this particular part of the work of the school, have been of great value.

Although it is impossible to obtain statistics regarding the relative degrees of speed and accuracy before and after the institution of the competition, those who are at work within the scheme are unanimous in their expressions of appreciation of its value.

On three occasions during a year, a private stimulus was offered to a House to make a special effort to improve its position, and it is notable that on two of the three occasions the House thus privately incited fell back from a position near the top to the bottom of the list showing the Houses in order of merit.

2. STATISTICAL RESULTS IN ARITHMETIC.

Tables I and II indicate the figures obtained from the school during one year.

Table I.

		Average number of points scored by the boys in the Form								
Year	Form	Sept.	Oct.	Nov.	Jan.	Feb.	Mar.	May	June	July
VIII	A	65·5	65·3	69·0	62·2	66·7	66·5	69·5	71·5	55·2
	B	59·2	56·2	62·3	64·7	63·3	68·8	74·3	78·0	64·0
VII	C	53·3	65·8	69·7	68·1	66·3	71·6	74·7	74·7	64·1
	D	52·8	58·5	58·2	60·1	57·3	57·3	67·1	66·1	58·8
VI	E	66·3	67·0	71·4	71·2	71·7	74·1	77·9	77·8	75·4
	F	60·2	74·9	78·7	75·8	79·3	80·6	87·3	89·2	88·3
	G	47·1	53·7	55·5	56·5	56·8	56·3	56·0	60·2	54 4
V	H	51·4	55·4	57·9	57·6	57·8	63·0	65·7	69·3	63·7
	I	53·7	54 7	60·1	58·9	60·6	62·5	67·0	70·9	67·7
	J	54·1	53·5	57·1	55·7	56·0	53·6	62·8	65·4	63·0
	K	43·5	47·6	49·3	50·4	48·3	59·5	52·3	55·0	52·6
IV	L	59·2	62·6	66·7	65·1	62·6	68·0	74·5	76·1	72·1
	M	52·5	53·4	56 4	52·4	60·1	57·0	65·9	70·6	66·2
	N	47·2	53·0	55·0	56·2	58·0	59·4	61·7	62·4	57·8
	O	47·7	52·2	54·7	54·1	55·5	51·8	58·7	59·7	60·2
	P	45·9	48·0	51·3	52·1	50·1	50·2	58·4	62·7	55·8
III	Q	54·6	62·7	66·0	60 7	61·6	64·2	69·1	72·8	69·4
	R	54·8	56·7	54·7	56·0	56·7	56·9	63·1	65·7	65·5
	S	48·0	50·0	51·7	52·6	53·7	54·7	60·7	63·0	61·5
	T	47·4	49·5	58·6	59·2	57·0	56·9	58·9	63·4	61·9
	U	42·5	47·5	50·2	53·6	53·4	54·6	58·0	59·4	59·2
II	V	51·9	59·1	66·7	66·9	73·1	77·4	79·3	88·1	83·0
	W	50·0	48·4	57·7	60·7	61·0	61·2	67·8	72·8	68·4
I	X	49·4	45·4	55·7	56·4	57·9	53·1	59 3	63·5	61·6

Table II. *Summary for year.*

	Aver. number taking the	Aver. age at end of year	Average number of points scored by boys in Groups									Percentage* of increase during
Year	test	y. m.	Sept.	Oct.	Nov.	Jan.	Feb.	Mar.	May	June	July	year
VIII	17	18 7	62·9	61·5	66·2	63·2	65·3	67·4	71·5	74·2	58·8	20·7
VII	21	17 5	53·1	63·4	65·9	65·4	63·3	66·8	72·2	71·8	62·3	36·0
VI	65	16 7	57·9	65·2	68·5	67·8	69·3	70·3	73·7	75·7	72·7	30·8
V	105	15 7	50·7	52·8	56·1	55·7	55·7	59·7	62·0	65·2	61·8	28·6
IV	131	14 5	50·5	53·8	56·8	56·0	57 3	57·3	63·8	66·3	62·4	31·3
III	116	13 2	49·5	53·3	56·2	56·4	56·5	57·5	62·0	64·9	63·5	31·1
II	50	12 1	51·0	53·8	62·2	63·8	67·1	69·3	73·6	80·5	75·7	57·8
I	24	11 4	49·4	45·4	55·7	56·4	57·9	53·1	59·3	63·5	61·6	28·5

* Measured from minimum to maximum.

The statistics given in Table I, in which the grouping into years is indicated on the left, were then averaged in years. To avoid the large amount of calculation necessary to obtain them more exactly, except in the cases of the seventh and eighth years, the average figure for the Groups of Forms was taken. (The arithmetical error thus introduced is not great, since each Form contained approximately the same number of pupils.)

This appears to have the advantage of eliminating variations due to the accidental absence or presence in a particular Form of individuals possessing a markedly high or low index.

Table II gives approximately accurate statistics for each of the years of the school course.

It is noticeable that, without exception, the indexes are lower in July than in June, as was also the case, with one exception, with the individual Forms. This may be a fatigue effect, but is more probably due to distractions occurring near the end of a school year, which also immediately follow First and Second School Examinations for many of the pupils and Term Examinations for the remainder.

Another noticeable point is the remarkable approximation to uniformity in the percentage increase during the year. The exception (year II) is that of a Group consisting largely of smart young pupils whose progress was quite remarkable.

It should be noted that the arithmetic indexes are percentages of a number of points higher than any pupil could attain, greater for a higher year than for a lower, but remaining constant during the year for any particular Form.

For the eighth year, for example, the maximum number of points was 400, whilst that for the second year was 200.

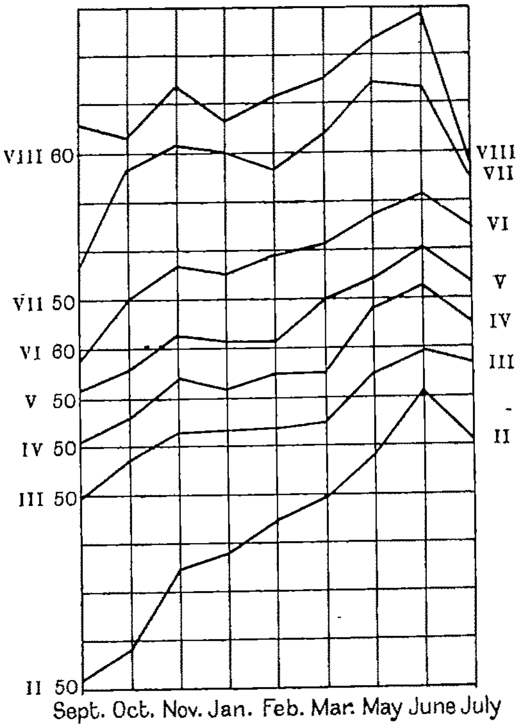

Fig. 1. Speed-Accuracy Indexes in Arithmetic. Each square represents 5 units. The unit from which the graph of each year commences is indicated on the left.

3. STATISTICAL RESULTS IN SPELLING.

Tables III and IV indicate the figures obtained from the school during one year.

Table III gives the Form statistics. The indexes for each Form are an average of the points awarded. In this case, as the words became better known during the year, a selection of more difficult words was gradually made.

Percentages were also obtained, as in the case of arithmetic, but the number of words required of each Form was gradually raised during the year, and the percentages did not therefore indicate the progress made as well as did the averages.

The use of averages does not indicate the whole progress, since the figures refer to quantities of gradually increasing order of difficulty. Whilst some amount of retardation is visible in the July statistics as compared with those of June, there is by no means the same pronounced fall as is evident in the case of arithmetic.

Table III.

Year	Form	Average number of points scored by the boys in the Form								
		Sept.	Oct.	Nov.	Jan.	Feb.	Mar.	May	June	July
VIII	A	85·6	87·3	101·7	103·8	105·8	104·0	105·0	107·0	106·0
	B	88·9	91·4	108·4	107·8	110·0	113·0	114·9	112·7	113·2
VII	C	82·6	82·9	98·3	104·8	101·7	98·4	108·5	105·8	107·0
	D	92·1	92·3	102·0	106·0	104·9	102·7	110·0	109·0	108·7
VI	E	78·6	80·7	94·8	95·9	99·5	94·3	97·0	99·6	100·5
	F	86·8	88·8	106·4	101·1	111·1	109·1	112·8	113·5	114·5
	G	68·2	74·2	81·7	85 8	85·7	86·6	95·3	93·7	96·8
V	H	66·9	68·9	81·4	82·4	85 9	83 9	90·3	89·1	92·4
	I	72·9	79·8	88·3	88·5	89·8	90·3	93·2	98·0	92·9
	J	61·6	66 7	72 7	78·5	79·3	80 9	82·9	86·6	86·7
	K	57·0	62·7	67·8	66·8	68·5	76·1	72 0	81·6	73·8
IV	L	56·2	62·7	71·1	75·3	74·6	80·4	79·5	86·2	85·7
	M	53·7	58·4	70·9	74·1	73·1	75·8	82·3	84·4	84·3
	N	48·7	55·5	64·1	65·7	70·0	70·9	80·0	82·3	79·5
	O	50·1	54·0	62·8	61·4	65·7	67 9	73·3	79·9	75·4
	P	42·6	47·0	53·6	51·9	56·4	55·1	57·0	64·5	64·9
III	Q	47·5	48·9	58·4	58·3	57·9	58·4	64·3	67·4	67·2
	R	43·2	44·7	55·6	54·3	53·6	54·7	60·0	65·8	62·7
	S	44·6	44·4	50 9	52·3	53·7	57·5	60·3	66·8	61·3
	T	32·4	36·8	51·5	42·8	47·2	41·7	45·6	46·3	49·9
	U	35·7	42·3	49·8	49·4	54·0	59·2	60·4	65·7	64·7
II	V	28·2	35·8	47·9	46·2	51·6	51·0	51·9	56·1	52·9
	W	30·9	30·5	37·4	36·6	40·4	39·5	44·6	47·5	46·7
I	X	28·9	32·0	41·3	40·4	40·8	39·4	34·4	37·5	38·7

Table IV. *Summary for year.*

Year	Aver. number taking the test	Aver. age at end of year	Average number of points scored by boys in Groups									Percentage* of increase during year
		y. m.	Sept.	Oct.	Nov.	Jan.	Feb.	Mar.	May	June	July	
VIII	17	18 7	87·0	89·0	104·5	105·4	107·5	107·7	109·1	109·3	108·4	25·6
VII	21	17 5	85·8	86 0	99 5	105·2	102·8	99 8	109·0	106·9	107·6	27·0
VI	65	16 7	77·9	81·2	94·3	94·3	98 8	96·7	101·7	102·3	103·9	33·4
V	105	15 7	64·6	69 5	77·6	79·1	80 9	82·8	84·6	88·8	86·5	37·5
IV	131	14 5	50·3	55·5	64·5	65·7	68·0	70·0	74·4	79·5	78·0	58·1
III	116	13 2	40·8	43·4	53·2	51·4	53·3	54·3	58·1	62·4	61·2	52·9
II	50	12 1	29·6	33·2	42·7	41·4	46·0	45·3	48·3	51·8	49·8	75·0
I	24	11 4	28·9	32·0	41·3	40 4	40·8	39·4	34·4	37·5	38·7	41·2

* Measured from minimum to maximum.

The case of Form X is peculiar. Several new pupils joined in May, and all obtained unusually low indexes. These have depressed the last three indexes of the Form as well as the percentage of increase during the year. For this reason graphs of the work of this Form are not included.

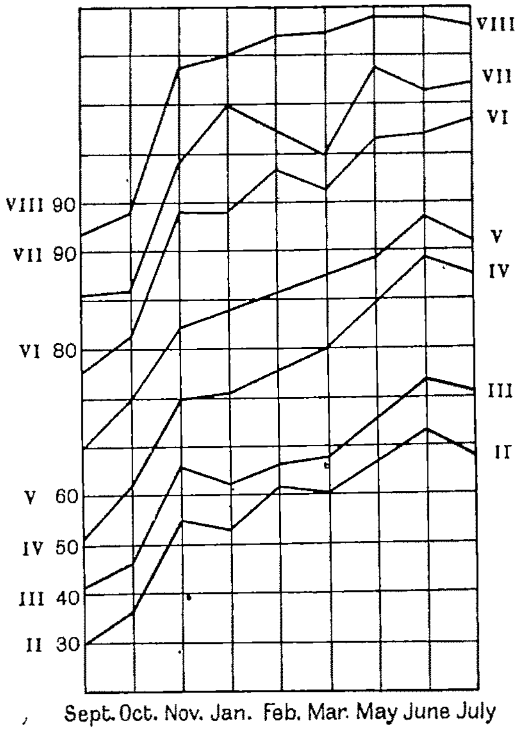

Fig. 2. Speed-Accuracy Indexes in Spelling. Each square represents 5 units. The unit from which the graph of each year commences is indicated on the left.

4. SEASONAL VARIATIONS.

A remarkable feature of the arithmetical graph, shared to a lesser extent by that of spelling, is the flattening which occurs in the months of January, February, and March. This may have been due to the attempt, previously mentioned, to apply a private stimulus to individual Houses. More probably, however, it may be due to a seasonal fluctuation in mental 'fitness' or efficiency.

It appears to be a well-established fact, although there is no solid detailed agreement among investigators, that both mental and physical efficiency fluctuate more or less rhythmically throughout a day and a year.

"For memory work and concentration of attention, the most favourable period is from October to January; then a decrease of mental power sets in, continuing to June and rising again from June onwards. Temperature and mental power appear to vary inversely[1]."

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[1] R. R. Rusk, *Experimental Education*, p. 248.

It would not, of course, be correct to say that the graphs of Figs. 1 and 2 confirm this statement in every particular. Having regard, however, to the fact that the quantities measured represent knowledge, skill, and alertness, and that from month to month these should increase, apart altogether from fluctuations in mental energy observable throughout a Term[1], the graphs would appear to indicate quite clearly that mental energy is greater in autumn and summer than in spring.

[1] This *Journal*, XIX, 448–451.

(*Manuscript received* 6 *December*, 1928.)

PUBLICATIONS RECENTLY RECEIVED.

Practice, Fatigue and Oscillation. A study of work at high pressure. By J. C. Flügel, B.A. *The British Journal of Psychology Monog. Suppl. XIII.* Pp. 92. 8*s*. 6*d*. net.

This is the record of an investigation by Mr Flügel in 1922 on 46 school children working at Kraepelin's adding sheets for 20 minutes on each of 46 successive week days. Among the principal conclusions were the following:

(1) The ability of the subjects showed steady improvement even toward the end of the experiment, and the usual phenomenon of a plateau was not discovered in their curves of work. The chief reason for this would appear to be that the increasing monetary rewards that were given to the children every time they broke their own records more than counter-balanced the monotony of the work.

(2) There was a correlation of 0·51 between initial ability and improvability.

(3) The more able subjects showed more variability from day to day than the less able.

(4) There was a considerable difference between individuals as regards fatigability, the more able individuals being on the whole less fatigable than the less able.

Unfortunately no conclusions could be drawn as to a natural periodicity in output. In order to make possible the system of marking, a special signal was given at the end of each minute and a special sign had to be written by the subjects in their books at the moment when the signal was given. The consequence was that the curves of work showed an artificial rhythm of more or less similar character in consonance with the minute signals, instead of a natural rhythm, different in each case. To obtain curves showing natural periodicity a series of experiments on slightly different lines would be necessary, but the present work is of great value, especially in demonstrating the effect of a system of incentives on the shape of the work curve.

Science and the Religious Life. A Psycho-Physiological Approach. By Carl Rahn. Yale University Press (and the Oxford University Press). 1928. Pp. x + 221. 13*s*. 6*d*. net.

This short book upon an enormous subject is unusual and noteworthy in some respects. It is not an attempt to explain religion in terms of psychology or physiology, as might be thought from the title, or an attempt to defend it against their encroachments. In fact, the author never puts himself to the trouble of giving any convincing proofs of the relation that he takes to exist between Science and Religion. He expresses his views, but does not state his reasons. This gives me access to the two criticisms that I wish to make. The real value in the book appears to be that it is a review of the present position of science, or, as he calls it, Naturalism, in order to try to judge where, and in what way, the pursuit of it is leading men. Mr Rahn appears to be afraid that contemporary Naturalism is failing to fulfil its promise to set up new standards in the spirit of inquiry, and hence in intellectual and moral freedom as well. His suggestions as to the possible nature and origin of this failure are very interesting, but in general his critique is not penetrating enough. As a consequence, however, of his view, which is never proved to be tenable, that physiology can explain all psychological phenomena, he claims that Naturalism (that is to say, physiology in this particular connection) must be a thoroughly inspiring ideal, since it can explain to us the biological value of religious experiences. If it is agreed to refrain from asking Mr Rahn to establish the soundness of his philosophy, the book remains an interesting sketch of some current problems.

The Symbolic Process and its Integration in Children. A Study in Social Psychology. By JOHN F. MARKEY, Ph.D. London: Kegan Paul. 1928. Pp. xii + 192. 10*s.* 6*d.* net.

The book contains a study of the nature and mode of development of word symbols in the individual; a short chapter, well worth reading, on the "delayed reaction"; and a clear exposition of the "stimulus-response" formulation when carried to its extreme position, including the view that "meaning" exists purely in the reaction. This is provoking in proportion as it is unconvincing.

Judgment and Reasoning in the Child. By Professor JEAN PIAGET. London: Kegan Paul. 1928. Pp. viii + 260. 10*s.* 6*d.* net.

It is very interesting to have a sequel to Professor Piaget's most valuable book, *Language and Thought of the Child.* Perhaps the greatest fault of the present book is that, while it contains an astonishing mass of carefully collected and analysed data, nowhere in his work does the author propound his conclusions in the shape of a simple and logical reconstruction. It is at the same time absolutely necessary and yet most difficult to make a reconstructive abstract of the work as one goes along, and if such an abstract is to be easily followed and apprehended it must be simple and logical. In this respect Professor Piaget has committed the error to which all true scientists are liable; he has allowed his ideas to float away into an ocean of facts. Thus, it is difficult to express the general contention of the work except in some such dry and paltry phrase as "the child does not think in the same way as the adult thinks." It seems that Professor Piaget need hardly have written two large books in order to discover anything so simple. Yet this is hardly fair.

A rapid review of the concluding chapter shows that the child, at the age at which Professor Piaget is interested in children, is neither perfectly play-like in his vocal activities, nor perfectly intelligible to an outsider in accordance with the standards of adult thought. He is, in fact, "ego-centric." This means a lack of conscious insight: he is not able to manipulate his thoughts in such a manner that each step and its relations to the other steps are well-defined, nor is he able to return on the path of his argument. Together with this there goes an inability to use logic—that is, the causal relatedness of thoughts—and hence a tendency to think in terms of accidental juxtaposition. This leads to a weakness of synthetic ability, for the thoughts are not balanced out according to their true relative values, but are thrown together anyhow. All this is really connoted by the term "syncretism," and the phrase "insensibility to contradiction." Professor Piaget speaks of the child as having an "intellectual realism," which is different both from an "intellectualism" and a "realism." This is difficult to comprehend, and yet it may be one of the most illuminating things he has said. But one feels that children must be much more variable than he supposes—perhaps even as variable as adults—in this respect. His final conclusion, that "causality," as understood by the trained and adult scientist, is not employed among the intellectual instruments of the child, is almost indisputable.

Instinct and Personality. By A. C. GARNETT. London: George Allen and Unwin. 1928. Pp. x + 218. 8*s.* 6*d.*

A. C. Garnett gives a good simple presentation and criticism of the theories of McDougall, Coué and the psychoanalysts, which he tries to synthesize into a complete dynamic theory of the organism. He holds that meaning is the guiding factor in any activity and that this consists essentially in "expectation"; thus "the forward looking teleological consciousness is the ultimate fact of existence"; expectation becomes the hormé or urge of life. The consideration of play and ideals shows that instincts cannot be regarded as the sole springs of action; moreover, the accumulation of instinctive energy is an untenable assumption. The author is also led to the difficult position of maintaining that all so-called unconscious mental processes are "purely physiological continuations of neural processes initiated by expectant attention." Various psychological data are reviewed in this light and a general monistic philosophy advanced.

The attempt is certainly suggestive, but difficult to evaluate. Expectation is a curious concept on which to build and one that must give rise to some confusion.

The reviewer would like to add that he has seldom come across a psychological book with so pleasant a format and type.

The Guidance of Conduct. By E. T. Dixon. London: Kegan Paul, Trench, Trubner and Co. Psyche Monographs, No. 2. 1928. Pp. ix + 219. 10*s.* 6*d.*

Despite its title this book is not another popular exposition of psychology applied to everyday life. From a logico-philosophical rather than from a psychological point of view the author urges that the sensations and ideas of associationism and traditional philosophy should be replaced by "formulae" whose function is to guide reactions. Thus words and images such as Value should not be hypostatized into entities that are assumed to represent realities, they are merely formulae symbolic of certain types of behaviour. Dixon's formulae are, of course, closely similar to the patterns of contemporary psychologists. This pragmatic theory is developed in relation to ethics, sociology and economics.

Types of Men: The Psychology and Ethics of Personality. By Eduard Spranger. Translated from the fifth German edition by P. J. W. Pigors. Halle: Max Niemeyer. 1928. Pp. xii + 402. R.M. 15.00.

The use of the notion of types seems to be returning to psychology in many different directions. Undoubtedly this is due, on the one hand to the revolt against the piecemeal, bit by bit study of human reactions, and on the other to an increasing attempt to deal psychologically with the higher mental functions. Especially in Germany "characterology" threatens to become the new psychological fashion, and numerous books purporting to discuss this topic in a scientific manner have appeared during the last few years and have met wide popular approval. Naturally these books are of very varying value, but Professor Spranger's is beyond doubt one of the most stimulating and valuable. To the more exclusively psychologically trained reader Spranger's outlook and work will appear far more philosophical than psychological, but his wide knowledge and his keen judgment constantly afford material of the greatest social and psychological interest. The book contains four parts: Philosophical Basis; The Ideally Basic Types of Individuality; Consequences for Ethics and The Understanding of Mental Structures. The second and fourth parts particularly contain a mass of acute psychological argumentation and observation. Most readers may wish often that a little rigorous condensation had been effected, but the book is in many ways a rich one, and will repay study, especially on the part of the social psychologist. The translation is good.

The "Soul" of the Primitive. By L. Lévy-Bruhl. Translated by Lilian A. Clarke. London: Geo. Allen and Unwin, Ltd. 1928. Pp. 351. 12*s.* 6*d.* net.

In this book Lévy-Bruhl applies his already formulated ideas concerning primitive logic to the "pre-notions" which he finds primitive people to possess of "their life-principle, their soul, and their personality." No new guiding principles of interpretation emerge, but numerous new illustrations are given within the realms dealt with.

The Child in Primitive Society. By N. MILLER. London: Kegan Paul. 1928. Pp. 307. 12*s.* 6*d.* net.

This is a well-documented study of the various ways in which the life of the child is directly shaped by social influences in a large variety of primitive social groups. It belongs to the type of book that throws a very wide net for illustrations, but all the sources are given and the treatment is perfectly clear. Not itself markedly original, it is the result of a great amount of painstaking study and will be of considerable value to the social psychologist who is in search of material.

Introduction to Social Psychology: Mind in Society. By R. MUKERJEE and N. N. SEN-GUPTA. Introduction by R. M. YERKES. London: D. C. Heath and Co. 1928. Pp. xv + 304. 7*s.* 6*d.* net.

This is a courageous and comprehensive introduction to the study of social psychology. The authors have made a genuine and in many ways a successful attempt to say where the real problems lie, and they have indicated lines of solution which any understanding reader may attempt himself to follow up. Fortunately, they have no cut-and-dried systematic schemes to offer. In fact the book is what it pretends to be, and students approaching this topic will find it a far more satisfactory introduction than most of the existing texts.

Emotion and Delinquency: A Clinical Study of five hundred Criminals in the Making. By L. GRIMBERG. London: Kegan Paul. Library of Educational Psychology, London. 1928. Pp. ix + 147. 7*s.* 6*d.* net.

This is a character study of a considerable number of delinquent girls, carried out at the New York Neurological Institute, and presented in a very readable way, with a fair collection of case histories. The girls studied appeared to fall into two groups. In the first were girls with no obvious adequate reasons for their delinquency. In general this group consisted of individuals who were mentally backward or dull, docile during early childhood, but ill-adapted to their home environment, and very often markedly selfish and liable to outbursts of temper. As a rule some special incident could be found which first definitely led to immoral or anti-legal action. In most cases there was complete absence of sympathetic religious training. The author is very definite that the downfall of these girls is rarely due to economic conditions. In the second group were girls morally completely unadapted to their society. Here, as contrasted with the first group, heredity appeared an important factor, pathological lying was very common. In general intelligence was low. Their occupational history showed a constant change of job. There was generally violent opposition between child and parents, and as a rule the career of delinquency was embarked upon earlier and before the home life was left. Girls in this group were almost always of marked and unstable emotionality, and their delinquency was mainly the expression of impulsive acts uncontrolled by cognitive factors. The author believes, and states his evidence clearly, that there is a definite "organic basis responsible for their condition." With the second group, as with the first, economic conditions were of minor importance.

The whole study is of great interest and should be followed up by an investigation, from a similar point of view, of possible remedial measures.

Emotions of Normal People. By W. M. MARSTON. London: Kegan Paul. 1928. Pp. xii + 405. International Library of Psychology, Philosophy and Scientific Method. 18*s.* net.

This book is undoubtedly a good deal longer than it need have been. At the same time it is written in a lively and interesting fashion, though the frequent colloquialisms may annoy some readers. It begins with an elaborate theory of consciousness, called "the psychotonic theory." Then, in the light of this, it discusses the principles of primary emotions, which are, according to the author: compliance, dominance, inducement and submission. The rest of the book is a more detailed study of these primary emotions, their mechanisms of expression, their functions, and, to a large extent, their characteristic differences in men and women. Much of this discussion is very clever and marks a genuine contribution to the subject. The author marshals his evidence well and deals with a mass of material that has been collected at first hand. The early part of the book is certainly open to the criticism of relapsing somewhat easily into a kind of speculative physiology.

Contributions to the Principles of Morphology. By W. B. CROW. London: Kegan Paul. 1929. Pp. viii + 94. 5*s.* net.

Dr Crow has produced a very clear and judicious contribution towards the study of scientific method. As he says, morphological and physiological methods are supplementary, though neither can be reduced to the other. The essay, after a general sketch of the development of morphological ideas and a discussion of the relations between observation and theory, deals with some of the leading ideas in the biological study of structure. The topics considered are: individuality, homology, symmetry and proportion and relationship by descent. Though the work does not bear directly upon specific psychological problems, a psychologist may learn much from its discussion of these leading notions.

The A.B.C. of Psychology. By C. K. OGDEN. London: Kegan Paul. 1929. Pp. x + 279. 4*s.* 6*d.* net.

After all, an alphabet is a late product of culture. It may reasonably be doubted whether psychology yet possesses one, or is likely to do so for some long time yet to come. However, the exigencies of a series may perhaps be held legitimately to determine titles. Even so, it is a little difficult to see why an A.B.C. should plunge the reader almost straight off into seven different theories of the relation of body and mind. The seventh theory, which appears to be the author's own, is called the "double language theory" and the main advantage of a linguistic solution would seem to be that it is less likely than the others to offend anybody. After this, however, the book proceeds rather well, and is clear, definite and well-informed. Too much is included, and not a little, as in the discussion of "asymmetry," could not, with the widest possible stretching of tolerance, be regarded as soundly established. But the reader will learn a lot and may be stimulated to try to discover more. A bibliography is added. The bulk of the books mentioned are those of a single series, but as most of them are very good indeed, that, perhaps, does not matter.

The Psychology of Language. By W. B. PILLSBURY and C. L. MEADER. New York: D. Appleton and Co. 1928. Pp. vii + 306. 10*s.* 6*d.* net.

Nobody who wishes to learn something about either the physiological or psychological factors involved in the production and use of language could do better than read carefully this sober and comprehensive book. After a study of the general scope of the problems involved, the authors give a clear and adequate account of the mechanism of speech production, and a most admirable discussion of the psychology of language. Recent experimental work is excellently dealt with. The final two sections, dealing with sound changes in speech and with syntax, are perhaps particularly noteworthy. Well selected references for further study are given at the close of every chapter.

Crossroads in the Mind of Man: A Study of Differentiable Mental Abilities. By TRUMAN L. KELLEY. Stanford University Press, California. 1928. Pp. vii + 238. $4.00.

Dr Kelley has here attempted an elaborate statistical study directed to the discovery and exposition of a method of proving the independence of mental traits. He argues, with much persuasiveness, that this kind of a study is the basis of a true understanding of human nature, and that it must precede the further study of how independent mental traits function in real life. His general position is well illustrated in the following quotation: "Mental life does not operate in a plain but in a network of canals. Though each canal may have indefinite limits in length and depth, it does not in width; though each mental trait may grow and become more and more subtle, it does not lose its character and discreteness from other traits." This thesis is pursued with unwearying skill through a mass of data statistically treated. An extensive bibliography is added.

The Soul Comes Back. By J. H. COFFYN. New York: The Macmillan Company. 1929. Pp. 207. 8*s.* 6*d.* net.

Professor Coffyn argues colloquially and vigorously against modern mechanistic and materialistic theories concerning human nature and destiny. His book is, as he frankly states, a venture of faith, and it may well appeal to the reader of robust moral activity.

The Varieties of Religious Experience. By WILLIAM JAMES. London: Longmans, Green and Co. 1928. Pp. vi + 534. 6*s.* net.

The cheap edition of William James' classical work should receive a very warm welcome. The book is extremely well printed and produced.

Whither Mankind? A Panorama of Modern Civilisation. Edited by CHARLES A. BEARD. London: Longmans, Green and Co. 1928. Pp. vii + 408. 12*s.* 6*d.* net.

In spite of a certain flamboyancy both in the style of production and in the initial claims made by the editor for this volume, it remains a collection of stimulating essays on various special questions in contemporary social life. After a general introduction by the editor, the topics dealt with are: The Civilisations of the East and West, by Hu Shi; Ancient and Mediaeval Civilisations, by Hendrik van Loon; Science, by Bertrand Russell; Business, by Julius Klein; Labour, by Beatrice and Sidney Webb; Law and Government, by H. L. McBain; War and Peace, by Emil Ludwig; Health, by C. E. A. Winslow; The Family, by Havelock Ellis; Race and Civilisation, by George A. Dorsey; Religion, by J. H. Robinson; The Arts, by Lewis Mumford; Philosophy, by John Dewey; Play, by Stuart Chase; Education, by Everett Dean Martin; and Literature, by Carl Van Doren. The editor contributes an epilogue. As will be seen from this list, the bulk of the book is by writers who can express themselves in an extremely competent journalistic style. This they succeed in doing, and naturally enough, though the essays differ greatly in value, many of them will most effectively challenge the thought and stimulate the investigations of the social psychologist.

Gotama the Man. By C. A. F. RHYS DAVIDS. London: Luzac and Co. 1928. Pp. 302. 4*s.*

In this book, which is cast into the form of an autobiography, Mrs Rhys Davids tells the story and unfolds the views of the founder of Buddhism. It is all written very simply and clearly, and reveals a most persuasive and attractive personality.

The New Psychology of the Unconscious. By C. W. VALENTINE. London: Christophers. 1928. Pp. xiv + 162. 4*s.* 6*d.* net.

Professor Valentine's admirable little book entitled *Dreams and the Unconscious* having been out of print for some time, he has reissued it under this new title. It remains as before, a restrained and acute presentation of some of the doctrines for which Freud was originally responsible, mainly in their relation to problems of normal psychology. The whole has been revised, and additional notes and illustrations have been added. The final chapter of this work is new. It deals with *Normal Individuals and the Influence of the Unconscious*, and takes up in particular the questions of repression in forgetting and dissociation in normal people. The book is intended for the general reader and is undeniably clear as well as just. It remains somewhat slight, and might well form the prelude to a more thorough study of psychoanalytic doctrines from the same point of view. It is much to be hoped that some day Professor Valentine himself will produce such a study, since this book is sufficient to show clearly that he can view exciting theories with a sane and balanced judgment.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

May 11th, 1929.	"Is there a common factor of sensorial rentivity and is it influenced by conation?" By Miss E. H. Walters, B.A.
„ „	"A balance showing the relation of blood distribution to mental activity" (with demonstration). By S. E. W. Taylor, M.A.
„ „	"Some factors involved in the performance of simple routine tests." By J. W. Caughey, B.A.

SECTIONAL MEETINGS.

Medical.

March 27th, 1929.	"Compulsive Thinking as a Castration Equivalent." By Dr Graham Howe.
April 9th, 1929.	A Symposium opened by Dr E. Jones and Dr Henry Yellowlees on "The Role of Anxiety in the Psychoses and the Psychoneuroses."
May 22nd, 1929.	"The Schizoid Personality—A Biological Variant." By Dr Emanuel Miller.

Education.

May 6th, 1929.	"Is there a Psychology for Modern Education." By R. B. Raup, Esq., Ph.D.
June 3rd, 1929.	"Individual 'Difficult' Children." By Miss E. M. Terry.

Industrial.

April 26th, 1929.	"Evidence of a Central Factor in Test of Manual Dexterity." By Mr J. N. Langdon, B.Sc.
June 17th, 1929.	"Some Psychological Problems in Gold Mining." By Dr G. H. Miles.

Aesthetics.

April 25th, 1929.	"The Work of an Aesthetics Group." By Miss Bodkin, M.A.

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BRITISH PSYCHOLOGICAL SOCIETY

RULES

NAME AND OBJECTS.

1. This Society shall be called the British Psychological Society.

2. The Society is instituted for promoting the co-operation of those interested in the various branches of Psychology.

3. The Society shall be responsible for the publication of *The British Journal of Psychology*.

MEMBERS AND SECTIONS.

4. The Society shall consist of Honorary Members, Ordinary Members and Sectional Associates.

5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.

6. Honorary Members shall be entitled to all rights and privileges of membership.

7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.

8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.

9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the meetings of the Section or Sections to which they belong.

10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology.*

10 (*a*). Transfer from Sectional Membership to Sectional Associateship may take place on January 1st only, and after not less than six months' notice from the Member of his wish to transfer.

11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of Members.

12. Every Candidate for election to the Society shall be recommended by at least two Members who shall be prepared to furnish information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next meeting of the Section respectively.

14. One full week before the meeting at which the ballot is to take place, the Secretary shall send to each Member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a Member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to subdivide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall

immediately take effect. But should the Council formally express disapproval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society summoned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES.

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, Chairman and Secretary of each of the Sections and of six Ordinary Members.

22. The President of the Society shall hold office for three years and shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. The other officers of the Society shall retire annually but shall be eligible for re-election.

23. Every year two ordinary members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of Members to serve on the Council may be suggested by the Council or by any Member by sending the proposed names to the secretary at least two weeks before the Annual General Meeting.

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each Member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council or by Ordinary Members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings, by ballot in accordance with Rule 15 at the next General Meeting, due notice having been given. Similar vacancies among the ordinary members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the members attending shall have power to transact business, subject to the approval of absent members, to whom the resolutions adopted shall be forwarded.

29. The first meeting of each Special Section shall be convened by the General Secretary of the Society. At this meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 23, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE.

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

34 (*a*). Ordinary Members who are members of all Sections shall pay an inclusive annual subscription of £2. 12*s*.

35. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This additional fee shall entitle members of any Section to receive such additional, and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psy-

chology studied by the Section. The amount of such additional fee shall be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

36. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

37. A Member may compound for his annual subscription by a fee, the amount of which shall be determined by the Council after consultation with the Sectional Committee and the Treasurer. Such composition shall entitle the Member to all the privileges of membership, which he enjoys at the time of composition, for life.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to Members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS.

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, or, failing him, a Vice-President, shall whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President or a Vice-President, some other Member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all Members of the Society notices of each meeting at least seven days beforehand, and of the business to be transacted at the meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty Members; at least fourteen days' notice of such meeting shall be sent to each Member of the Society.

VISITORS.

45. Each Member shall have the privilege of introducing two visitors at any meeting. The Council or a Sectional Committee shall however have the power of closing any meeting or meetings to visitors, provided notice to this effect be given on the notice convening such meeting or meetings. The names of visitors together with the names of Members introducing them shall be entered in a book provided for that purpose. Any Member of the Society attending a meeting of a Section to which he does not belong must be introduced by a member of that Section.

BRANCHES.

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of Ordinary Members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as Ordinary Members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of Members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

RULES.

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.

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VOLUME XX OCTOBER, 1929 PART 2

TESTS OF TEMPERAMENT AND PERSONALITY

BY P. E. VERNON.

(*From the Cambridge Psychological Laboratory.*)

1. *Introduction: temperament and personality* (pp. 97–98).
2. *The Downey Will-Temperament Test* (pp. 99–102).
3. *Fundamental drawbacks to the objective test: necessity for a qualitative method of direct observation* (pp. 102–104).
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Summary (pp. 116–117).

1. INTRODUCTION: TEMPERAMENT AND PERSONALITY.

THE aim of a test of temperament or personality should be to predict, on the basis of a subject's reactions to a simple, well-defined test situation how his reactions in real life are likely to be affected by his temperamental qualities and personality traits. By 'temperament' I mean, roughly speaking, the total organization of an individual's innate tendencies of the affective and conative type, as distinguished from innate sensory and mental abilities that are more specifically cognitive and from definitely motor capacities[1]; if one accepts a McDougallian theory of action it includes also individual differences in instinctive equipment. Possibly these tendencies are determined by bodily characteristics such as physique, endocrine activity, etc., though that is hardly a psychological problem. In the adult, at any rate, temperament cannot be observed pure; 'personality' is to be regarded as the functional resultant of temperamental qualities, of intelligence and other characteristics of the organism as influenced and overlaid by environment and training. The term 'character' has, it seems to me, a non-psychological meaning that is too firmly established, namely an assertive or determined personality, to be adopted in psychological discussion. At present one can scarcely be more specific with one's definitions; I believe that the most satisfactory solution as to their nature, if not the only solution, is to be obtained by individual and comparative psychological research along the lines of the direct observational method of approach described below. However, we can realize, to some extent, their extreme importance; for we can measure, theoretically at least, a subject's mental abilities and manual aptitudes for various skilled tasks, but a knowledge of these is insufficient for prophesying his success and efficiency in our present social, educational and industrial systems since it omits the affective and irrational influences of inherited and acquired emotional dispositions such as carefulness, persistence, 'push,' timidity, emotional instability, also sentiments and interests which essentially determine what he will make of his talents. Thus Bartlett[1] defines temperament as "a group of predisposing tendencies and feelings which may be used to characterize one person in contrast to another, and which seems to come to expression independently of special training...." Temperamental qualities "determine a man's attitude and responses towards any number of situations rather than to a single specific situation..., they begin to find expression extraordinarily early in individual mental life, and they appear to be capable of singular persistence from generation to generation."

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[1] Cf. C. Burt, *The Measurement of Mental Capacities*, 1927, Henderson Trust Lectures, No. VII, Edinburgh.

In actual life, when we wish to gauge the temperament or judge the personality, say, of a schoolchild or a candidate for a job, we generally use the ordinary personal interview; but this is far too subjective and unsystematic to be trustworthy. However broadminded and dispassionate we attempt to be we cannot avoid the 'halo' effect, hence the technique of the rating method has been elaborated in an attempt to cancel out our personal prejudices and similar attendant disadvantages. It can seldom be said to have succeeded practically owing to the difficulty of getting suitable raters; in fact one must admit that the method often throws more light on the personality of the rater than the ratee; it is also too cumbersome to be serviceable outside the psychological laboratory. Hence there have developed, chiefly in America, innumerable tests of temperament and personality which claim to be scientific and objective, not depending on the vagaries of personal judgments. From the psychologist's point of view, however, the criterion for a good test must, ultimately, be the extent to which its results agree with those of trustworthy ratings.

I intend in this paper to consider one of the best known of these tests, to indicate some of its drawbacks and then to describe another method which is a compromise between the unsatisfactory extremes of the objective test and the subjective interview.

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[1] F. C. Bartlett, "Temperament and Social Class," *Eugenics Rev.* 1928, xx (1), 25–28.

2. THE DOWNEY WILL-TEMPERAMENT TEST.

The June Downey Will-Temperament Test, it is asserted, gives fairly accurate and objective measures of twelve traits. These are deduced chiefly from the subject's handwriting, since Downey[1] tries to show that temperament is expressed not so much in its shape and details (the graphologists fallacy) as in its speed, modifiability and the like. This theory also finds support from Galton[2] who states that the handwriting of identical twins is often the only measurable difference between them, so that "it would appear that the handwriting is a very delicate test of difference in organization." The details of the series of separate tests and the traits that they are meant to measure are familiar by now and need not be described again here; they have been summarized recently by Collins[3] and Oates[4] in this *Journal*. The traits fall into three groups, firstly those indicating speed and fluidity of reaction, secondly aggressive qualities, thirdly carefulness and persistence. They are marked out of 10 on the basis of empirical norms and are then plotted on a graph called the will profile whose shape is said to be highly characteristic of each subject's reactions in ordinary life. The total score for all the traits should indicate general strength or weakness of personality.

The test has been thoroughly tried out and there have been one or two moderately successful attempts to explain disparities between scholastic success and intelligence scores by means of it; also it is said that independent judges can sometimes recognize whom the will profiles represent. But on the whole it is most unsatisfactory for reasons that I hope to demonstrate, and most investigators only allow that it deserves notice as a pioneer effort, and that, on the analogy of early tests of intelligence, it is likely to lead to better results in the course of time.

Some of the main criticims are as follows:

(*a*) A general drawback to the whole series of tests, that can hardly be avoided, is that Downey's directions and norms for scoring are quite unsuitable for English adults; my subjects were of a fairly homogeneous type, hence their variation in performance was small, and their average mark, especially in the tests for Speed of Movement, Flexibility and Co-ordination of Impulses, was well above Downey's published average, namely about 7 marks out of 10 instead of 5[1]. This means that every investigator has to work out his own arbitrary norms in order to obtain adequate comparative measures of his subjects' temperamental traits. There are also several difficulties in the details of the application of the tests which each investigator has to solve for himself; some of these are given by Collins[2], but there is no need to recapitulate them since there are many other more fundamental objections.

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[1] J. Downey, *The Will-Temperament and its Testing*, New York, 1923.

[2] F. Galton, *Inquiries into the Human Faculty*, 1883, p. 157 in 1907 ed.

[3] M. Collins, "Character and Temperament Tests," this *Journal*, 1925, XVI, 89–99.

[4] D. W. Oates, "An Experimental Study of Temperament," this *Journal*, 1928, XIX, 1–30.

(*b*) The tests are unreliable; Rugg[3] gives 0·4 as the probable average correlation between two applications of the test to the same subjects. We scarcely realize the significance of such a coefficient; in another set of experiments I obtained this figure between two sets of scores for 150 subjects, so that it was nearly 20 times its P.E., and yet 2 per cent. of the subjects showed differences of 85 per cent. in their two scores, 10 per cent. differed more than 60 per cent., 20 per cent. differed more than 40 per cent. and over a half differed 25 per cent. or more. In other words, taking a physical analogy, this is as if one measured the boiling point of water one day and found it to be 100°, and on the next day there was an even chance of its being 68° or 132°.

In a single application of the tests for Freedom from Load and Motor Inhibition several trials are given in which I noticed highly divergent results that cannot be accounted for as practice effects; in the former there was an average difference between the first and second sets of trials corresponding to 2 marks, one way or the other.

(*c*) The correlation with ratings, as worked out by other investigators is very low: May[4] gives an average of 0·1 to 0·24 according to the methods used. The ratings themselves are, however, quite unreliable, so that this only proves that the raters do not know what the traits are supposed to mean. Hence the tests are of little or no practical or diagnostic value, but are still merely a psychological curiosity. I tested 23 subjects myself, with whose temperament and personality in ordinary life I am familiar; from consideration of their performances there often appears to be some correspondence with their characteristic qualities and traits, yet there is almost as much gross disagreement. This fault I hold to be inherent in the whole conception of the objective test of temperament, one that is unlikely to be eradicated however much further work is done on tests of this type.

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[1] D. W. Oates (*op. cit.*) also raises this criticism, though, as regards these particular traits, his schoolboys, tested by the group form of the test, were below, not above Downey's average.

[2] M. Collins, *op. cit.*

[3] H. G. Rugg, "The Downey Will-Temperament Test," *Journ. App. Psych.* 1923, VII, 65–76.

[4] M. A. May, "The Present Status of Will-Temperament Testing," *Journ. App. Psych.* 1925, IX, 29–52.

(*d*) Many of the tests seem to depend on motor capacities and habits, not psychological traits. Speed of Movement and Freedom from Load are almost certainly improved by experience in writing essays or taking lecture notes. Flexibility and Interest in Detail are affected by practice in drawing and copying. Motor Inhibition is helped by practice with microscopic work, dissection, drawing in detail, also by the possession of a large handwriting. A high score on Co-ordination of Impulses must to some extent depend on the natural speed and size of writing, though Downey denies this; I find that it certainly goes with a capacity to play violins or other stringed instruments, and so probably depends on practice, though of course Downey might assert, conversely, that this temperamental quality helps in playing a stringed instrument. Volitional Perseveration probably depends on habitual slowness[1] and lack of intelligence. If this muscular bias be admitted, some of the tests do seem to measure something besides themselves, especially Speed of Movement, the second part of Flexibility (the capacity for rapid imitation of an unfamiliar handwriting) and Motor Impulsion.

(*e*) Several of the tests depend largely on subjective judgments for their scorings; thus it is already acknowledged by the deviser that all the traits that constitute her conception of will temperament cannot be measured objectively. These more definitely psychological estimates seem to agree even less with real life and with ratings. The fundamental reason for this is that scarcely any account is taken of the mental state of the subject, the meaning the situations have for him. Speed of Decision and Finality of Judgment, for example, set up an intensely complicated attitude in the subject. He is asked to rate himself on a number of character traits, or to underline one of a pair of traits that best describes himself; apart from the obvious differences that will arise according to which alternative he adopts, the results will depend on his self-consciousness, his inhibitions, his lack of conscientiousness (which can hardly be identified with Downey's "speed of decision or lack of rambling and excessive caution"), the amount of self-analysis to which he is used, and, especially, his personal relations with the investigator. In the tests for Flexibility and Volitional Perseveration that require the disguising of his handwriting, the determining subjective factors are equally incalculable. The tests for Reaction to Contradiction and Resistance to

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[1] M. Collins (*op. cit.*) comes to the same conclusion as regards this test.

Opposition are particularly unsatisfactory, and depend also on the attitude of the subject to the investigator. In the former the subject chooses one of two envelopes and is later contradicted as to his choice; none of my subjects acknowledged that they had forgotten their choice (as they should have if they had been very unaggressive), but also, quite naturally, none of them troubled to maintain their opinions (as they should have if they had been very aggressive), the situation being an intrinsically futile one. In the latter, where they write with their eyes closed, and are supposed to react aggressively or submissively to an obstacle placed in the way, they usually thought that their pencils were moving off the paper and that I was guiding them; hence, by the use of more appropriate directions, the subjective situation had to be carefully prepared beforehand. The test for Motor Inhibition (including its four practice trials) would often take an hour or more to perform and may therefore be affected by such irrelevant factors as the subject's desire not to be late for lunch[1].

Half the traits depend on the use of tricks in the tests (Speed of Decision, Contradiction, Opposition, Finality, Interest and Perseveration), such as timing the subject when he has been told that he has unlimited time; I cannot believe that this practice is valid; for one thing, few stopwatches are silent, and innumerable, indefinable subjective factors invariably enter and distort the reactions.

It may well be that the fact that my subjects formed, on the whole, an intellectual, and therefore somewhat selected group, is responsible for the poor results I obtained. Working with children other investigators often seem to be more satisfied than I am. But surely it is hardly psychologically sound for a test to rely on the credulity and lack of sophistication of children and, apparently, the average American experimental subject.

3. FUNDAMENTAL DRAWBACKS TO THE OBJECTIVE TEST: NECESSITY FOR A QUALITATIVE METHOD OF DIRECT OBSERVATION.

I maintain that the whole objective method rests on a fallacy that the *Gestalt* psychologists call the Constancy Hypothesis, namely that identical, standardized objective situations or external stimuli are equivalent to similar standardized subjective ones, both for one person at different times and for different people at the same time. Psychological testers seem to think that definite and invariable test directions and conditions should be aimed at when it is clear that these will produce entirely different subjective situations in different people. In fact, by trying to be scientific, their methods are essentially unscientific; they are acting as a physicist who might try to prove Joule's law using different apparatus at every attempt, a mercury thermometer one day, a hydrogen one the next, and so on, without considering any of the vagaries of these instruments. Orthodox scientific method cannot take into account the variability and the sources of inaccuracy in psychological material, namely the human mind in all its complexity. The basis of more enlightened scientific method is systematization of the phenomena with which it deals, not the neglect of the factors it cannot easily control. It is gradually being realized that this type of experimental psychology forgets the individual and that a more clinical type of approach is needed. Even an ideal set of tests of temperament and personality which attained perfect reliability and which agreed exactly with external ratings would only establish an artificial statistical individual, as Viteles[1] puts it, a mosaic of "isolated sectors of the individual ego." Similarly, Allport[2] recognizes that personality traits merely represent groups of characteristic reactions of the individual selected and abstracted for consideration; personality or temperament is a co-ordinated whole, an integration or configuration of all these abstractions that cannot be put into objective terms or measured satisfactorily.

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[1] This difficulty can be overcome by substituting the group form of the test in which, instead of taking as long as possible to write 'United States of America' (up to 30 minutes), the subject traces a scroll with his pencil, doing as little as possible in a definite time (2½ minutes with ½ and 1½ minutes practice trials).

Temperament is, in fact, qualitative rather than quantitative; we ought to consider the way a subject reacts in a number of situations rather than how much he reacts in a specific situation, as shown by Bartlett's definition, given above. Burt[3] and other educational psychologists have elaborated a method whereby they simply observe children's modes of reaction while answering specifically devised questions and in situations such as Binet testing, or even during expeditions to the shops, the cinema or the Zoo. From these objective observations they deduce the children's temperaments subjectively, intuitively one might almost say, and by this empirical method they are able to obtain results that are reliable and of immediate and practical value. Even the Downey tests can be used to give several good qualitative indications of this type; for example, in Speed of Decision, using a rather less embarrassing set of character traits, I gave up trying to time the test secretly but observed each subject's carefulness, frankness, tendency to quibble, etc., and also took some account of his actual self-estimates, judging whether he seemed fair or biassed. Even more significant was the way in which he did the Motor Inhibition test, his self-control, unsteadiness and remarks. It was also possible to supplement these indications by noting the amounts of scroll covered in successive half minutes; those who found the test the greatest strain and who, from other observations, tended to be generally temperamentally unstable, often varied as much as 30 per cent. in their speed. It should also be possible to devise good tests for Resistance to Opposition and Interest in Detail in which there is no trick but in which the subjects' normal reactions could be qualitatively observed.

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[1] M. S. Viteles, "The Clinical Viewpoint in Accident Prevention, Symposium on Accidents," *Proceedings of the Vth International Psychotechnical Conference, Utrecht,* 1928.

[2] G. W. Allport, "The Study of the Undivided Personality," *Journ. Abn. Soc. Psych.* 1924, xix, 132–41.

[3] C. Burt, *The Young Delinquent,* London Univ. Press, 1925.

I seem to be advocating a regression from the attempted scientific methods of the supposedly objective test to the subjective inaccuracies of the personal interview. The difference from the ordinary interview, however, is that this approach is, as far as possible, systematized and controlled, for the test performances to be observed provide definite objective standards on the basis of which different subjects may be qualitatively compared; specific types of behaviour emerge during these performances which can be recorded objectively and so serve to minimize the investigator's or interviewer's subjective prejudices. The actual performances chosen do not matter very much as long as they give plenty of opportunity for the subject to show himself up; they must be such that the whole personality takes part in doing them, and so becomes mirrored in the resulting behaviour and can then be recorded. Hence a simple motor test is not likely to be of much use in that it may often be performed as an automatic and specialized habit. Naturally the ideal would be for the interviewer to observe closely each subject's reactions in ordinary life over a long period, but if a number of different tasks are provided the whole process can be considerably shortened, the method will be much more practical and need not become artificial, and at the same time may be more objective and unbiassed than such protracted observation might tend to be.

4. APPLICATION OF THIS METHOD WITH PERFORMANCE TESTS.

As a tentative exploration of this method I collected and gave to 12 of my Will-Temperament Test subjects a number of performance tests and games, of a simple subjective nature, namely:

1. "Anchor" BRICKS (No. 13, "The Tormenter")[1]. A number of small, angular bricks of uniform height, but different shapes, are to be built into a variety of two-dimensional patterns, squares, polygons, etc., all degrees of difficulty being available. They take from about 1 to 25 minutes to accomplish.

2. GUIDIT. A small steel ball is chased up an inclined plane to a goal at the top with a knitting needle; in the plane are several holes past which the ball has to be guided, there are also various diagonal barriers round which it must be taken, so that progress becomes more and more difficult as the top is approached. If the ball falls through a hole or off the edge it must be started again from the bottom.

3. A LOCK and door handle mechanism has to be assembled from about nine component parts. Those who are unfamiliar with locks take about 20 minutes over it.

4. DESIGNS. A number of pieces of card, triangles, squares, etc., of ten different colours are provided. The subject chooses whichever of these he likes and arranges them on a white ground into any pattern, design or picture which he regards as the best he can produce with the available material.

5. CUBES. Fifteen cubes of wood (sides ½ in.) have to be built up in a pile on top of one another. The sides are badly cut, neither being at true right angles with each other, nor even strictly plane, hence this is practically an impossible task.

6. An ordinary postcard size JIGSAW Puzzle, representing a landscape.

7. The Downey SCROLL test for Motor Inhibition, as described above in the note on p. 102.

These tests were not given in a laboratory, nor under strict experimental conditions. Either I showed them to a subject in order to see if any spontaneous interest was evoked, or else asked him to "play some games" for me, for psychological purposes. I usually explained that I was not interested in objective measurements of his quickness (thus a first success at GUIDIT occupies from 5 to 40 minutes, with practice 2 minutes is a fair average, so that the actual time taken has very little temperamental significance), but said that I wished him to do them quite naturally so that I could see in what way he did them, whether he liked them or not, and found them easy and interesting or difficult and irritating, and so on. I then simply observed whether any obvious qualitative individual differences existed in the performances of various subjects, and, in spite of the many difficulties of this method, a number of temperamental traits showed up immediately, ones which I know and which, other people agreed, the subjects exhibit in ordinary life.

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[1] I should like to take this opportunity of expressing my thanks to Miss Fildes, who gave me several useful suggestions and told me of this test and of the Cubes (No. 5), which she has previously applied to children with similar results.

Each subject must come at least twice; half-a-dozen times would be desirable in order that he may be given as many opportunities as possible of displaying his temperament from different aspects. While he attempted the test I wrote down as many notes as to his behaviour, mood, etc., as I could, keeping separately the actual facts of observation and my immediate and subsequent deductions. It is essential for the investigator to try to eliminate any previous opinions about his subject's personality. His mind must be as alert and fresh as possible, he should try to enter into each subject's thoughts and feelings, to understand, simplify and control the subjective situation. Allport[1] shows that great individual differences exist between different investigators in their intuitive capacity, and suggests that the personalities of their subjects must be apprehended empathically.

Hence each subject should be treated differently; to one the whole nature of the test may be explained, this would put another on his guard, and he might, for example, repress his habitual emotionality, therefore he must not be enlightened quite so much. A third should be stimulated to concentrate on the tests, a fourth should be allowed to smoke and talk freely, and so on. The question of giving hints when the test goes on too long, or suggesting that subjects should or should not give it up is also a very variable and subjective one. Undoubtedly far more standardization of instructions, method, etc., is desirable, without their becoming stereotyped and artificial; at present the procedure is so indefinite that too much depends on the good faith of the investigator, and how far he can overcome his halo effects. There is also a considerable danger of drawing conclusions about a subject's temperament from one test, and only taking full note of reactions to subsequent tests that fit in with these conclusions.

But there is certainly a very pleasing degree of objectivity in the test, namely that so many actual remarks and reactions can be recorded, and reconsidered subsequently till subjective biases are overcome; introspection on the part of the subject is unnecessary (an important point to remember in view of a possible application of the method to education or industry), and, when it was asked for, it only served to confirm the objective observations already obtained. The method is, in fact, closely similar to that of Köhler[1] who set his apes several problems and watched to see how they would solve them, his deductions being based not on any actual measurements, but simply on the behaviour of the animals and his intuitions as to their feelings. I myself was frequently struck by the resemblance of my subjects to the apes who, when thwarted by some obstacle from obtaining food, expressed rage or despair or became thoroughly bored with the situation until, suddenly, the true solution dawned on them and was carried out rapidly and completely. The apes also showed similar temperamental differences in their reactions to all the test situations, hence Köhler is able to describe their personalities.

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[1] G. W. Allport, *op. cit.*

A further obvious drawback to these tests is that they are games which will be reacted to differently from situations in real life. Actually it appeared that it was no use trying to perform them in this dissociated way; in constructing the lock, for example, one's mind soon becomes absorbed (that is, if one's mind is temperamentally ever capable of concentrating on anything), every possible item of experience or method of solution that might help is resuscitated. So that though the objection might apply to one or two of the tests, yet when a subject can be proved to show the same traits in several situations on different occasions, it seems fairly justifiable to assume that these same traits are likely to appear in his reactions in ordinary life.

5. RESULTS OBTAINED.

My subjects were too few in number to make it worth while determining statistical correlations between external ratings and the results of tests; moreover, it is most difficult to put these results into adequate quantitative terms. Hence, in the present investigation, I had to rely on my own observations in ordinary life and discussions of the personalities of my subjects with other people as standards against which their behaviour in test situations could be compared. I will therefore abstract certain traits that seemed to emerge most obviously and describe shortly the reactions of those who exhibited their presence or absence in the most extreme form.

Every test gives some indication of a general but important trait, Constructive Interest, creativeness or inventiveness; perhaps with McDougall we should call it an instinct. To be able to test for it would be most valuable, for those who are deficient in it would be best adapted for the monotonous work so common in modern industry, those who are superior in it can never be satisfied with such automatic tasks. It may be remembered that Köhler's apes often showed definite aesthetic tendencies in the form of aimless, playful reactions, adorning themselves with rags, branches and the like. Nueva even managed to construct a primitive knot without any utilitarian purpose, and Sultan, when he had succeeded in making a double stick with which he could reach the fruit outside his cage did not stop to eat it but pulled in all sorts of useless objects.

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[1] W. Kohler, *The Mentality of Apes*, 1925, translated from 2nd edition by E. Winter.

Subject A. is typically of what is called the artistic temperament in ordinary life. In doing GUIDIT, although he showed great emotional instability (see below), he was spontaneously 'fascinated by it.' He chose the other tests more or less in order of constructiveness, namely DESIGNS, JIGSAW, LOCK, BRICKS, CUBES, saying, for example, that he preferred LOCK to BRICKS "because it has more variety."

At DESIGNS he lost all sense of time and went on spontaneously for two and a half hours at one sitting; during this time he completed about 20 designs, fully exploiting the most striking combinations that the other subjects hardly attempted. He continually said, "I do love this game," again, "I like this game, there are no mechanical principles." His enthusiasm prevented me from becoming at all bored at merely watching him, and though, naturally, any investigator will have particular preferences, I certainly appreciated his colour combinations more than those of any other subject. He appeared to be more interested in colour schemes than in form, the latter seeming to be implicit and effective without needing conscious consideration; he judged the colours that went together well subjectively (in Bullough, Valentine and Myers' sense). He showed distinct empathic tendencies, *e.g.* arranging darker colours near the bottom and on the side farthest from the window, or else, by combinations of light and shade, obtaining remarkable three-dimensional effects. His symmetries were always vertical and diagonal rather than horizontal, thus if AB, CD, etc., represent various patches of colour or groups of coloured pieces, he used schemes such as $\begin{matrix} AB & BA \\ CD & DC \end{matrix}$ or $\begin{matrix} AB & AB \\ DC & DC \end{matrix}$ rather than the more conventional $\begin{matrix} AB & BA \\ \multicolumn{2}{c}{C} \\ AB & BA \end{matrix}$. He had no wish to make representational designs or pictures and deprecated the suggestion.

The JIGSAW he built up characteristically with reference to the whole picture, synthetically rather than analytically. In doing the BRICKS he frequently tended to design his own patterns, forgetting the set ones. Two methods of balance are possible in the CUBES, either the angles may be carefully examined and complementary ones fitted together so as to produce a straight pile, or else successive cubes may be made to overhang the ones below them in order to compensate for badness of fit, producing what one might call a 'crazy chimney' effect. A. adopted the latter and took pleasure in the result as such.

At the opposite extreme subject D., who is characterized by somewhat prejudiced and unimaginative artistic tastes, was only interested in the tests when immediately successful at them (see below, Persistence), and showed none of these aesthetic tendencies. Similarly, B., E., I. and K., though not lacking in artistic taste, are seldom if ever aesthetically creative in real life; they were incapable of producing satisfactory abstract designs and tended, instead, to make representational ones, pictures of buildings, boats and the like. Subject I. said, "I cannot imagine a design being pleasing" and proceeded to amuse himself and me with trees, birds and houses.

Contrasted with the aesthetic tendencies are the mechanical or scientific trends of several personalities; thus the LOCK appealed most to E. and K.; the former, though he was quite unfamiliar with this type of mechanism, had put all the pieces together except the spring in 4½ minutes, and after carefully working out the principles completed it in about 9 minutes, half the time taken by any other subject. Similarly, by means of close consideration of the angles of the CUBES he obtained a pile 13 cubes high at his first attempt, and his average height before falling was 12, the more normal figure being 8 to 9. The BRICKS constitute the best single test since there is opportunity for exhibition of both aesthetic and scientific tendencies. E. was again the subject who made most use of analytical and geometrical principles, always effectively working out the proportions of the various pieces instead of using overt trail and error methods, or, as most of the subjects, depending on the attainment of correct configurations by chance.

We have seen already how artificial are the abstractions we call temperamental traits; each one that I consider overlaps, positively or negatively with every other. Hence scientific Constructiveness is not very different from what might be called Planning Capacity. This is not the same as intelligence, at least agreement with marks obtained on specially devised intelligence tests was nil; but the agreement with ratings of the same subjects on intelligence was fair, so that some kind of intellectual power rather than alertness and brightness is indicated by these performance tests. Subjects G. and H. demonstrated very clearly the lack of it; the former started off on GUIDIT with a very inflexible method of balancing; she quite failed to realize that once the ball starts to run down the needle, say to the right, so that the right-hand end is raised to stop it, that directly after the left-hand end must be raised to prevent the ball running back in the opposite direction. Hence her failures differed from those of the better balancers in that the ball fell off the edge of the plane much more frequently than into the holes; usually the reverse holds. Again she made no attempt to take account of the irregularities of the surface of the plane at different places. She constructed the JIGSAW more or less blindly, taking but little account of the whole, and was very slow at realizing when pieces did not fit into each other, often forcing the wrong ones together. In the BRICKS she continually made the same mistakes; though often getting the pattern almost correct, by chance, she failed to see the logical solution. One of these patterns was a square, and on arriving, fortuitously, at a rectangle she thought she had succeeded and took several seconds to realize the mistake. She always built up the patterns in small pieces, with no relation to the whole. Yet she differed from H. in being interested and alert throughout.

H. yawned frequently and sighed; she similarly exhibited poor balancing reactions in GUIDIT; in the LOCK she never seemed to think out the functions of the parts but tried the pieces, blindly, with listless movements and without any active interest. G. had remarked that she preferred BRICK to LOCK since, "I should never be able to do it, even if I saw you take it to pieces," while H. soon gave up saying, "My mechanical brain is not very good." The latter was equally lacking in any 'geometric sense,' for example she was unable to see that a certain right-angled triangle was equal in area to a certain square, so that by slight readjustment she could have at once completed one of the patterns. Instead she pulled the pattern to pieces, and, by doing it by trial and error, failed to get it right. It may be noted that they both scored very low on Downey's Interest in Detail test, though the agreement between this and Planning Capacity was not very close.

They may be contrasted with K., who was mentioned above, or with C. who looked at the pieces often for 1½ minutes at a time, thinking out their proportions, etc., rather than using trial and error methods. Similarly, he considered the coloured pieces for 3 minutes before he began making a DESIGN. He carefully analysed the irregularities of the surface of the plane in GUIDIT, remarking, "I must plan out the best way first"; his movements were slow and controlled, and he rolled up his coat sleeves in order to attack it better. His method of building the CUBES and his successes were similar to those of K. This trait also seemed to be related to the subject's degree of concentration on and attention to the tasks; some tended to get bored and talk about irrelevant affairs, others showed an active, fighting attitude. The latter, when held up, often tapped the table or floor with their fingers or foot, or whistled softly.

Closely connected also, though perhaps more in the nature of muscular abilities, were the manual control and steadiness in CUBES and GUIDIT.

Though they are not wholly mutually incompatible, a trait that one may call Emotional Instability is, to some extent, the reverse of Planning Capacity. This bore a fairly close relation to one of Downey's best tests, that for Motor Impulsion or impetuosity. But emotional traits are very easily obscured by the suppression that the subject manages to maintain, and hence on his attitude towards the investigator; presumably more searching and prolonged tests of a suitable kind would eventually break it down, if it is ever broken down in ordinary life.

Subject A., to whose 'artistic temperament' I have already referred, exhibited the trait to a high degree. In the Downey SCROLL he worked steadily for short periods at 2 units per half minute, but when engaged on it for 2½ minutes he continuously quickened up, reaching 4 units in the last half minute, saying at the end that it "made me feel all quivery." His score for Motor Impulsion was 8.

In GUIDIT he averaged at least 4 to 5 falls a minute; he often knocked the ball right over the top of the plane on to the floor several feet away by rushing it up too quickly or pressing it violently against the barriers. At almost every fall he cursed or thumped the table; he made frequent remarks about his unsteady hand and showed every sign of exasperation. I neither interfered nor stimulated him in any way and, with occasional periods of self-control and steadiness, he spent 40 minutes in obtaining his first success. While doing the BRICKS he alternated between emotionality and seriousness, sometimes breaking into hysterical laughter when the pattern turned out to be wrong. He pulled these unsuccessful attempts to pieces childishly and started again from the beginning. One or two of his remarks were, "It is irritating that the bricks are not all of the same colour and surface," and "This is a short cut to madness."

He showed considerable variability on different days, but though comparatively quiet while doing the cubes the average height of his piles was only 7. In the more constructive tasks, LOCK, DESIGNS, JIGSAW and in making his own patterns with the BRICKS he was far calmer.

Subject I., on the contrary, was absolutely steady with the SCROLL, and drew it more slowly than any other subject, namely at 0·225 units per half minute. In GUIDIT he only averaged 1½ falls a minute; his movements were slow and controlled and he reached the top in 14½ minutes. He often whistled under his breath while doing the BRICKS, and though lacking in Planning Capacity, he attempted to work out the patterns geometrically, and did not show emotional or wilful movements. When given the CUBES he said, "I am going to do this in spite of you"; "You will not easily make me get annoyed, I do not normally do so"; he laughed at his falls but without exasperation and took great care in picking out the firmest ones for the base.

K.'s emotional stability has already been mentioned under the heading of scientific Constructive Interest. The nearest approach to a complaint or exasperation of any kind was a remark about the BRICKS, "This must be very ingenious," and "I am sorry I am taking such an awful lot of time over this."

Though it is not easy to give verbal illustrations, yet there certainly seemed to me to emerge two emotional types, on the one hand those who maintain an even level of moderate emotional reactions, and on the other hand those who show great variations and inconsistencies, alternately becoming very uncontrolled and then quiet again; these variations or lack of variations appear in different tests on different days or in the course of one test[1].

Some indications of the commonly accepted *extravert-introvert* distinction were obtained; subjects B., D., E., H., I. and K. would be acknowledged by any external judge to be predominantly extravert in everyday life. All of them, and none of the rest of my subjects, showed the characteristic of asking how their performances compared with those of other people, or else what I thought of their performances. B., for example, in GUIDIT said, "How far do people usually get with this?", "This must be boring for you"; in the CUBES, "What is the record with this?"; H. and K., similarly, with GUIDIT asked, "Does anyone ever get to the top?" and "Don't people reach the top earlier than this?" Another characteristic, shown especially by E., H. and K. was a marked *suggestibility* in GUIDIT. About one-quarter and half the way up are two places that are especially difficult to pass; some time after starting the test these subjects tended to get stuck at these places, even if they had passed them several times before; H., for instance, spent 5 minutes, failing at the lower hole and 4 minutes at the upper, while once she had broken the spell and regained her self-confidence she had hardly any difficulty with them. It is clear that the influence of competition and other temperamental factors connected with social and gregarious qualities could be demonstrated by further work on tests of this type. Thus F. showed continual tendencies to childish emotional reactions and exasperation, but when a third person entered the room (though paying no attention to the experiments) he controlled himself far more, while the opposite effect was observed once with subject A.

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[1] This same distinction is noted by M. D. Vernon, "On Certain Effects of Long Spells of Repetitive Work," this *Journal*, 1926, XVI, 222–36. One of her subjects showed the same emotional variability characteristics in her tests and mine.

Finally there is the important trait of Persistence, also closely bound up with Planning Capacity; the actual times spent on the tests before giving up seems to me to depend on a complex of factors such as (*a*) actual personal interest in the task; some may be more persistent at the BRICKS than at the less constructive GUIDIT, though in general the same degree of Persistence did seem to be exhibited in all the tasks; (*b*) self-assertion or vanity that makes subjects object to being beaten by a simple test; usually (*a*) starts at a high level, dwindles and (*b*) keeps them going; they alternate between satisfaction, hope and despair. A few develop more of an aesthetic or Constructive interest as they go on. (*c*) Indirect interest, *i.e.* the extent to which the subject thinks that I expect him to go on, how well his performance stands with reference to those of others, etc. The investigator should of course do his best to standardize this, so that it may exert the same subjective influence on all the subjects. At the extremes, however, I think that it played very little part. Take, for instance, D., who notably lacks Persistence and determination in ordinary life. At first he was highly self-confident and was delighted with GUIDIT; but after only four falls, at 3 minutes, he said, "I am not very successful at this sort of thing." A minute later he nearly reached the top and said, "I like this game" and "This is rather a good game." But 3 minutes later he gave up spontaneously with, "Here, I think I'd better try your lock"; he had only had 9 falls altogether, as contrasted with A.'s 150 to 200 falls. He added that he would find the lock easy as he was interested in physics. But in 1½ minutes, "I have no idea how these things go. This beats me"—2 minutes, "I have never seen anything like this before." He fumbled hopelessly, not even trying to work out how the key fitted, although quite intelligent. At 6 minutes, "This means absolutely nothing to me," then he hedged and rationalized, "Now if you had given me wires to solder, I am good at that." At my suggestion he was very glad to give up and try the BRICKS, and was extremely pleased at completing an easy pattern in 1½ minutes. When given a harder one he soon tended to talk about irrelevant matters. At

3½ minutes, "There do not seem to be enough pieces to make this one" and murmured (not for me to hear), "Impossible." He gave up at 5¼ minutes.

E. and K., on the other hand, only asked whether I did not get bored at watching them for such a long time. E. spent 8 minutes fitting in the final piece of the LOCK, without showing any signs of giving up. When given a very difficult BRICKS pattern he remarked at 18 minutes, "So far no solution, but let's have another try," and took 25 minutes in all. When asked he replied, "I would have gone on much longer but might have been rather bored." K. at the LOCK apologized for taking such a long time, and at 17 minutes said, "Do you mind if I go on and finish this?" He succeeded in 23½ minutes without any tendency to give up. He built up piles of cubes for 21 minutes till I stopped him. After taking 22 minutes for a first success at GUIDIT he asked if he was to continue.

Such big differences as this can hardly be put down to subjective influences exerted by the investigator. It may be noted in this connection that psychologists make very bad subjects for the tests; for example, subject I. remarked while doing the BRICKS, "I must do the normal thing," "Is it persistence you are getting at?", "You are wondering if I am going to give up sooner than the others." Again, at the CUBES, "It is no good your trying to measure my persistence, as I can go on at this any length of time." Their experimental bias makes it impossible for them to approach most of the tests naturally, though DESIGNS was fairly free from these artificial factors.

6. CONCLUSION.

We see, then, that it is possible, with the aid of these tests, to obtain a very complete picture of each subject's temperament, and that the method is capable of considerable development and practical application. Thus I could guarantee, with BRICKS, LOCK and DESIGNS, to diagnose in about an hour whether aesthetic or scientific tendencies are predominant in any subject, without any introspection on his part or previous knowledge of him. There was a close agreement between the aesthetic qualities demonstrated in the tests and the artistry, emotional attitude, etc., in musical appreciation. I also suggested in an earlier article[1] that a definite personality type existed among men who are successful at rowing; it was characterized by suggestibility, determination and persistence, extraversion and various social qualities enabling them to fit in well with other members of their boat and of the Boat Club. I now feel confident of being able to diagnose the essential traits with the aid of my tests and with parts of the Downey Will-Temperament Test, since the latter often indicates useful motor qualities, even if it throws scarcely any light on more psychological factors. E. and K. are fairly representative of the type and their characteristics have been shown above to conform excellently with these specifications; they are fairly suggestible and extraverted, emotionally stable and persistent; nor are they too aesthetically inclined or introspective; their motor control in GUIDIT and CUBES was good. In the Downey tests good rowing men should not obtain any very high or low scores[1]; but their Speed, Load, Impulsion and total scores should be rather above the average, and their Motor Inhibition steady. Nearly half my Will-Temperament test subjects were rowing men, their profiles showed distinct tendencies to similarity in that their variability on any one of the sounder tests was much lower than that of the other half of my subjects. I was not familiar with K.'s rowing characteristics, but I managed to deduce from all the tests that he was somewhat slow in the boat, but was very persistent, and not easily upset or hustled, also that, socially, he got on very well in any boat that he was in; these deductions were entirely confirmed by subsequent enquiry.

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[1] P. E. Vernon, *The Psychology of Rowing*, this *Journal*, 1928, XVIII, 317–31.

But these experiments were, of course, tentative and inadequate; more tasks and more subjects are required; tests could be arranged to give many more objective indications. The results depend at present too much on the investigator; his judgments should also be compared with the ratings of those who base their estimates on the characteristics of each subject in ordinary life. But I feel justified in maintaining that this approach is more satisfactory than any of the more objective methods, such as the Will-Temperament test, that have been devised so far, in that it attempts to take account of the subjective situation, is less artificial and is more likely to lead to results that may be used in the actual practical diagnosis of personality and temperament. Further tasks that might be used are, firstly, essays or discussions on set problems, and secondly, some kind of apparatus on which the subject might be given cognitive tasks in the performance of which he would exhibit his temperamental qualities; in neither of these need the real object of the experiment be mentioned and more natural behaviour might be expected.

In conclusion, I found unexpected support recently from a number of continental psychologists[2]; in many German and other laboratories temperament is determined by this direct observational or qualitative method applied in any of the psychological experiments that various subjects undergo[1]. For example, Nagy[2] maintains the following points: in the testing of temperament conditions should be as normal as possible; qualitative, not quantitative, tests should be used, relative not absolute principles should be applied; the subjective situation is the most important, and the investigator should use intuition as well as intellect, so that "Ein guter Psycholog muss nicht nur ein Gelehrter, sondern auch ein Künstler sein."

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[1] Cf. D. W. Oates' (*op. cit.*) conclusions with regard to success at school work.

[2] *E.g.* L. Fejgin-Garstensteyg, *Certaines Manifestations du Caractère et du Tempérament dans les Épreuves Psychotechniques*, and J. Suter, "Ueber die Ermittlung von Charaktereigenschaften bei der psychotechnischen Prüfung der Fähigkeiten," *Proceedings of the Vth International Psychotechnical Conference, Utrecht*, 1928.

SUMMARY.

(1) Temperament and personality are of extreme social, educational and industrial importance so that it is very necessary to devise satisfactory methods of diagnosing, estimating and measuring them, and of establishing more clearly their psychological nature.

(2) The rating method is scarcely practicable, although preferable to the ordinary personal interview.

(3) The following main criticisms may be raised against objective tests such as the June Downey Will-Temperament Test:

(*a*) The directions, details of application and norms are unsuitable for English adults and have to be worked out afresh by every investigator.

(*b*) The statistical reliability of the component tests is very low.

(*c*) The agreement with ratings is usually so poor as to make the test of little if any practical value.

(*d*) Many of the tests depend on motor capacities and habits rather than psychological traits.

(*e*) Others are not applied and scored really objectively.

(4) In general such tests neglect the complexity of the subjective factors involved; owing to their very objectivity and scientific orthodoxy they are artificial and psychologically unsound.

(5) Personality and temperament are organized and integrated wholes which cannot be put into simple quantitative terms; their estimation must be supplemented by direct qualitative observation and intutition.

(6) Another approach, intermediate between the objective test and the subjective interview, as used by Köhler, Burt and in several continental laboratories, was tried with far more promising results.

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[1] I believe that similar methods are used practically in the vocational guidance work of the National Institute of Industrial Psychology.

[2] L. Nagy, "Zweck und Methode der padagogisch-psychologischen Prüfungen," *Proc. Vth Int. Psychotech. Conf.* 1928.

(7) Various simple performance tests and games were given to a number of subjects under conditions that were as normal as possible. Their remarks and behaviour were observed and many traits emerged.

(8) Chief of these were aesthetic and scientific constructive interest, planning capacity, emotional instability and variability, extraversion-introversion, suggestibility and persistence.

(9) They appear to agree well with the subjects' characteristics in real life, and the method is capable of considerable development, systematization and practical application.

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GROUP FACTORS IN TEMPERAMENT QUALITIES

BY DAVID W. OATES.

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3. *Examination of the inter-correlations of the tests* (pp. 119–123).
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6. *Summary and conclusions* (pp. 135–136).

1. INTRODUCTION.

IN a previous paper[1] the present writer reported the results of an experimental study of temperament. The tests then used were critically examined, the temperament profiles obtained from 50 subjects were studied, and the possible relationship of temperament as tested by these tests to intelligence and scholastic success was examined in the light of scores obtained in the temperament tests by successful and unsuccessful students respectively—success being indicated by school achievement. The conclusions there reached were sufficient to suggest that the investigation should be continued to provide data for a more intensive analysis of the traits measured by the tests. The present paper is a direct continuation of the previous investigation; and its special object is to examine more fully the nature of the temperament qualities functioning in the tests, with a view to discovering the possible existence of one or more group factors.

2. SOURCES OF DATA.

(*a*) *Temperament tests*. The tests themselves were taken from the Downey Will-Temperament series, described in detail in the previous paper; the following were selected:

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[1] This *Journal*, July 1928, XIX, 1–30.

T 1. Speed of Movement.
T 2. Freedom from Load.
T 3. Flexibility.
T 4. Speed of Decision.
T 5. Motor Impulsion.
T 6. Self-Confidence.
T 7. Non-Compliance.
T 8. Finality of Judgment.
T 9. Motor Inhibition.
T 10. Interest in Detail.
T 11. Co-ordination of Impulses.
T 12. Volitional Perseveration.

(*b*) *Scholastic tests.* The subjects were students in a Secondary School. The marks obtained by them in two School Terminal Examinations, held at an interval of six months, provide the data for scholastic estimates. Other tests were administered during the period intervening between the two examinations.

(*c*) *Intelligence tests.* For estimating intelligence a battery consisting of the ten following tests was used: Obeying Orders; Mixed Sentences; Analogies; Number Series; Best Reason; Common-Sense; Cypher; Meaning of Words; Absurdities; Orientation.

(*d*) *Handicraft tests.* The medium of the Temperament Tests is handwriting executed under varying conditions. Burt[1] has shown that writing has a fairly high specific correlation with handwork and drawing; and has suggested that all three may involve a special ability or 'group factor,' doubtless consisting largely in an aptitude for skill and speed in finger movements, not unlike what is sometimes called motor ability. In order to test the possible relationship of the Temperament Tests to the motor factor present in handwork, a number of practical tests in woodwork consisting of marking out and making models in wood were carried out by the subjects of the investigation in the Handicraft Department, and the marks obtained for the exercises were used for calculating the correlations which appear below.

3. EXAMINATION OF THE INTER-CORRELATIONS OF THE TESTS.

The results summarized in Table I appear to indicate that there is little if any relationship between temperament and intelligence as measured by the tests used in this investigation. The coefficient of 0·111 becomes a partial coefficient of – 0·159 when the influence of school examinations is eliminated. This agrees with the results based upon the data reported in the previous paper[2] and with Stead's results[3] on an investigation in

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[1] C. Burt, *The Distribution and Relations of Educational Abilities*, pp. 58 and 61.

[2] This *Journal*, XIX (1), 17.

[3] H. G. Stead, "Factors in Mental and Scholastic Ability," this *Journal*, XVI, 3–207. Cf. A. A. Roback, *The Psychology of Character*, 1927, p. 156: "Neither intelligence nor volitional qualities enter into the temperamental make-up of a person; and on this point it is my impression that all are agreed."

Table I. *Inter-correlation of tests.*

	T 1	T 2	T 3	T 4	T 5	T 6	T 7	T 8	T 9	T 10	T 11	T 12	I	E 1	E 2
T 1	—	·614	·130	·117	·173	–·189	–·063	·187	–·027	·112	·308	–·020	·141	·019	·194
T 2	·614	—	–·083	·486	·158	–·010	·120	·296	–·100	·132	·322	–·046	·159	·349	·508
T 3	·130	–·083	—	·127	–·153	·010	–·197	–·034	–·183	·386	·049	·075	·078	·050	·219
T 4	·117	·486	·127	—	–·184	·168	·066	·227	–·081	·275	·435	·129	·071	·298	·488
T 5	·173	·158	–·153	–·184	—	·165	·043	–·034	·020	·084	·087	–·033	·025	–·018	·078
T 6	–·189	–·010	·010	·168	·165	—	·083	·205	–·142	–·037	–·003	–·350	·284	·253	·386
T 7	–·063	·120	–·197	·066	·043	·083	—	·032	–·110	–·039	·169	·150	·275	·303	·123
T 8	·187	·296	–·034	·227	–·034	·205	·032	—	·216	–·006	–·093	–·061	·174	·183	·294
T 9	–·027	–·100	–·183	–·081	·020	–·142	–·110	·216	—	·135	·047	·007	–·178	–·186	–·038
T 10	·112	·132	·386	·275	·084	·037	–·039	–·006	·135	—	·382	–·075	·247	·412	·445
T 11	·308	·322	·049	·435	·087	–·003	·169	–·093	·047	·382	—	–·045	·176	·401	·513
T 12	–·020	–·046	·075	·129	–·033	–·350	·150	–·061	·007	–·075	–·045	—	–·119	–·217	–·187
I	·141	·159	·078	·071	·025	·284	·275	·174	–·178	·247	·176	–·119	—	·589	·539
E 1	·201	·349	·050	·298	–·018	·253	·303	·183	–·186	·412	·401	–·217	·589	—	·811
E 2	·194	·508	·219	·488	·078	·386	·123	·294	–·038	·445	·513	–·187	·539	·811	—

which he used estimates for certain character traits where we have used the scores made in tests of temperament qualities. The T tests in Table I which have fairly high correlation with intelligence apparently involve some amount of intelligence in their performance. Test 6, in the form here used, as the author points out, "encourages an intellectual balancing of reasons rather than the immediate characteristic response[1]." The quality involved in Test 10 is measured by the exactness with which a model is copied, and Test 11, which correlates definitely with Test 10, also tests ability to act correctly as well as the ability to act quickly. This element of correctness involves the exercise of an intellectual factor. Test 7 again is related in type to Test 11 and correlates highly with it. Tests 1 and 2 probably correlate with intelligence on account of the speed element. Most of the intelligence tests are worked with definite time limits, and speedy workers are likely to do well in these tests and also in T tests 1 and 2. Such relationship between the temperament tests and intelligence as the figures in our table indicate, is probably to be attributed to the influence of certain temperamental qualities upon the performance of subjects in the intelligence test, rather than to the existence of an intelligence factor in the T tests themselves. We may assume, therefore, that intelligence does not enter as a general factor into the T tests as a whole, but that it operates to a slight extent in a few of the tests only.

Most of the T tests have a higher average correlation with the two school examinations than with the other T tests; for example, T 6 has a coefficient of 0·320 with the examination results and – 0·026 with the other T tests, and T 10 has coefficients of 0·429 and 0·129 respectively. The correlation between the school examinations and temperament tests, therefore, suggests the possibility of a factor common to both. If '*g*' is not a general factor in the T tests, as our results above appear to prove, the general factor common to the results of the school examinations and the results of temperament tests is something other than '*g*.' I suggest it is some general temperamental factor, enabling the subject to put more energy into his examination work, and incidentally no doubt to work at higher speed.

The high correlation between examinations and intelligence indicates that a general intellectual factor runs through both. This is a result that would be expected, for school examinations have been proved to be highly saturated with '*g*.' We conclude, therefore,

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[1] Downey, *The Will-Temperament and its Testing*, p. 124.

(1) that examination results and intelligence correlate on account of a general intelligence factor which we identify with Spearman's '*g*';

(2) that examination results and temperament also tend to correlate on account of a general temperamental factor which is closely related to Burt's central emotional factor[1] and may even be identified with it.

This last suggestion is offered tentatively: the T tests are not constructed so as to call into play all the various emotions the measurement of which would be necessary to obtain a good estimate of 'general emotionality.' On the other hand, there is the possibility that some factor closely related to the process of writing is operating both in the T tests and in the school examinations. This view, however, cannot afford a complete explanation of the correlation coefficients. Four tests, 4, 6, 7 and 8, involve no writing; and yet show a fairly high positive correlation with the examination results, the average coefficients being 0·393, 0·320, 0·213 and 0·238 respectively; only three of the tests involving writing have higher coefficients, while five have definitely lower coefficients. It appears clear, therefore, that the general factor is not to be identified directly with the writing process, but is some wider temperamental factor which has close affinity to 'general emotionality.' No doubt in the temperamental tests ease and speed of writing may be an incidental symptom of 'general emotionality.' This conclusion is supported by the statement of Burt, who writes: "In the few rough experiments I have made the correlation between quickness of habitual handwriting and quickness of general emotional response proves high enough to warrant tentative deductions along these lines. In a group of 143 students the correlation proved to be 0·57[2]."

We may now examine our table of coefficients to determine if the general factor which we suggest above is an adequate ground of explanation of the relations indicated by our coefficients. It will be sufficient for our purpose if we take the four tests T 1, T 2, T 3, T 4 and the two examinations E 1 and E 2, and calculate the value of all the possible tetrad differences. The following result was obtained:

Table II. *Tetrad differences.*

Tests T 1, T 2, T 3, T 4, E 1, E 2.

Mid-point of F/P.E.

±0·5	1·5	2·5	3·5	4·5	5·5	6·5	7·5	8·5	9·5	10·5	11·5
28	25	14	7	3	5	2	—	2	—	2	2

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[1] C. Burt, "General and Specific Factors underlying the Primary Emotions," *British Association Report*, 1915.

[2] "The Measurement of Mental Capacities" (*The Henderson Trust Lecture*, Oliver and Boyd, 1927), p. 20.

In a normal distribution only 45 cases should fall outside the limits of ± P.E., here 62 do so; 23 cases exceed ± 3 P.E. where about 6 cases would be expected. This distribution clearly exceeds what can be attributed to sampling errors, and indicates that, in addition to any general factor which may be present, there is a group factor or group factors operating it may be in the T tests or in the E tests, or in both. Any one of these conditions may be sufficient to account for the distribution obtained.

There appears, therefore, to be a definite indication of the existence of a group factor or factors in the T tests. We may examine this possibility by calculating all the possible tetrad differences which can be obtained from the inter-correlations of the twelve T tests. This table was given in the previous report[1], from which it will be seen that 8·7 per cent. of the tetrad differences lie outside the limits of ± 3 P.E., whereas only 0·7 per cent. should do so, and 6·2 per cent. lie outside ± 5 P.E. where we should expect only 0·1 per cent. This clearly indicates that there is correlation between the T tests other than can be attributed solely to a common factor running through them all, and thus proves the existence of a group factor or factors.

4. EXAMINATION OF THE DATA FOR THE PRESENCE OF GROUP FACTORS.

The temperamental tests are arranged by their author into three groups each consisting of four tests measuring (1) speed and fluidity of reaction, (2) forcefulness and decisiveness of reaction, and (3) carefulness and persistence of reaction. This arrangement may be regarded as implying the existence of three group factors in temperament qualities. The suggestion is further strengthened by the recurrence of the terms 'rapid-fire type,' 'aggressive or forceful type,' and 'deliberate, careful type' in Dr Downey's analysis of profiles, the types being determined by high scores in each of the three groups of tests. How far the author deliberately intended to convey such an interpretation of her grouping it is difficult to say: possibly the arrangement is merely designed for convenience of classification. Certainly, she makes no reference to group factors; and probably she intends these three groups to be understood as revealing different aspects of temperament. This seems suggested when she writes, for instance: "Profiles frequently reveal only a slight emphasis of one or two *aspects of temperament*; sometimes they are best described in negative terms as non-aggressive or careless[2]."

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[1] This *Journal*, XIX, 26.

[2] Downey, *loc. cit.* p. 71; the italics are ours.

We may examine our table for any evidence supporting a suggestion that there are group factors of speed, of drive, and of care-persistence present in the three groups of tests designed to test these qualities. The four speed tests have an average correlation of 0·232 and are definitely correlated with the exception of the slight negative correlation between Tests 2 and 3. The average correlation among the tests forming this group is higher than the average inter-correlation with the tests of the other two groups, which is 0·110 and 0·036 respectively. The tests in the aggressive group are correlated to a slight extent with a low negative correlation between Tests 5 and 8. The average correlation, however, is only 0·082. In the carefulness-persistence group Tests 10 and 11 are definitely correlated but Test 12 shows no indication of relationship to the other tests in the group. The average inter-correlation for the group, 0·075, is lower than the average inter-correlation with the tests in the speed group, 0·110, in fact Tests 10 and 11 have high correlation with the tests in this first group. There seems little evidence to support the suggestion that there are group factors underlying these three groups of tests; even in the group in which the tests appear to have the strongest tendency to hang together two of the three possible tetrad differences exceed ± 2 P.E. We may conclude, therefore, that the qualities measured by the tests of speed and fluidity, aggressiveness, and care-persistence do not represent the functioning of group factors.

From the nature of the T tests it may be suggested that any super-correlation which they may reveal is due to the considerable degree of motor ability which they involve. Investigations of the simpler motor activities, such as tapping or tracing, have failed to indicate that these activities are positively correlated[1]. This may not hold, however, for more complex activities involving freer and more highly co-ordinated movements. Burt[2], for example, as we have seen, found that writing, drawing and handwork are specifically correlated, and was led to suggest the existence of a group factor in such abilities, which he has termed a manual factor. The existence of a similar factor may be looked for in our T tests inasmuch as eight of them involve writing and another tracing. The actual inter-correlations, however, do not point to this conclusion, and it is significant to note that the correlation of the T tests with handicraft exercises is, with the single exception of test T 3, Flexibility, lower than the average correlation of each of the T tests with the two school examinations, as is seen in the following table:

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[1] Cf. Perrin, "An Experimental Study of Motor Ability," *Journ. of Exper. Psychol.* 1921, IV.

[2] *The Distribution and Relations of Educational Ability*, p. 58.

Table III. *Correlation of T tests with handicraft tests and school examinations.*

Test	Exam.	Handicraft	Test	Exam.	Handicraft
T 1	·197	·078	T 7	·213	·139
T 2	·429	·219	T 8	·239	·049
T 3	·135	·463	T 9	–·112	–·219
T 4	·393	·247	T 10	·429	·249
T 5	·030	–·042	T 11	·457	·009
T 6	·320	·178	T 12	–·202	–·047

In handicraft motor ability and activity are definite determinants of success in the exercises worked; there is apparently, therefore, something more than a pure motor factor operating in most of these tests.

Our search for possible group factors will be assisted if we rearrange our table of coefficients as far as possible in descending order of magnitude with tests having positive correlation grouped together at one extreme and those with negative correlation grouped together at the other extreme. This may be done systematically by calculating the correlation of each test with the hypothetical general factor running through them all and arranging them in the order of these coefficients. The following table results:

Table IV. *Inter-correlation of tests.*

	2	4	11	10	1	8	5	7	3	9	12	6
2	—	·486	·322	·132	·614	·296	·158	·120	–·083	–·100	–·046	–·010
4	·486	—	·435	·275	·117	·227	–·184	·066	·127	–·081	·129	·168
11	·322	·435	—	·382	·308	–·093	·087	·169	·049	·047	–·045	–·003
10	·132	·275	·382	—	·112	–·006	·084	–·039	·386	·135	–·075	·037
1	·614	·117	·308	·112	—	·187	·173	–·063	·130	–·027	– 020	–·189
8	·296	·227	–·093	–·006	·187	—	–·034	·032	–·034	·216	–·061	·205
5	·158	–·184	·087	·084	·173	–·034	—	·043	–·153	·020	–·033	·165
7	·120	·066	·169	–·039	–·063	·032	·043	—	–·197	–·110	·150	·083
3	–·083	·127	·049	·386	·130	–·034	–·153	–·197	—	–·183	·075	·010
9	–·100	–·081	·047	·135	–·027	·216	·020	–·110	–·183	—	·007	–·142
12	–·046	·129	–·045	–·075	–·020	–·061	–·033	·150	·075	·007	—	–·350
6	–·010	·168	–·033	·037	–·189	·205	·165	·083	·010	–·142	–·350	—
Hypothetical general factor	·651	·606	·572	·488	·462	·322	·109	·087	·045	–·076	–·091	–·098

We may first consider the coefficients which are apparently out of place in the hierarchical table. The coefficients which fall into this class are T 3 and T 10 0·386, T 8 and T 9 0·216, T 7 and T 11 0·169, T 4 and T 6 0·168, T 7 and T 12 0·150. An examination of the remaining correlations in relation to these indicates by the number of negative coefficients occurring that there is apparently no question of a group factor involved in the irregular coefficients arising from these tests; the explanation is probably to be found in the actual activities involved in the tests themselves. In the first case, Tests 3 and 10, this is clearly the cause as both tests involve the imitation of a model. The explanation of the fairly high positive correlation between Tests 8 and 9 is not so clearly apparent. The positive correlation is somewhat unexpected; Downey suggests[1] that Test 8, Finality of Judgment, would have negative correlation with Test 9, Motor Inhibition, though she says elsewhere that Motor Inhibition which "is given first place in the final group might be included among the aggressive traits[2]" in the previous group. Many subjects it is suggested manifest a tendency to return again and reconsider their judgments and thus spend an excessive amount of time in revision: those who score high in Test 8 have to inhibit this tendency, and the positive correlation in our table may be explained by the element of inhibition called for in working the tests. In the case of Tests 4 and 6 the latter reveals a form of self-assertion characteristic of the forceful individual who might be expected to be speedy in making a decision. Tests 7 and 12 also appear to involve somewhat similar qualities; as the author of the tests remarks: "the deliberate, careful type assert their opinion more diffidently but hold to it more tenaciously[3]." The perseverator, therefore, might be expected to reveal resistance to opposition.

We may assume, therefore, that these irregular coefficients in our table are determined by the actual test activities involved, and proceed to consider the implications of the table as a whole. There are apparently two groups of tests which show evidence of relationship. At the positive end of our table are five tests which have positive correlation with one another throughout, and are either negatively correlated with other tests or have zero or positive correlations which are too irregular to have significance for our purpose. Of these five tests, 2, 4, 11, 10 and 1, three of them, 2, 4, 1, are definite tests of speed, while Test 10 involves the rapid imitation of a model, and Test 11 is scored upon the ability to write in a restricted space in speeded time. The conclusion seems inevitable, therefore, that there is a general factor of speed entering into these tests, and the suggestion is strengthened by the fact that the highest coefficients of correlation occur between tests which are most obviously speed tests; scores in Tests 1 and 2, for instance, are determined by the speed of normal writing and the ability to speed up the act of writing. Similarly, Tests 2 and 4 measure the ability to write rapidly and the ability to make a rapid decision.

At the other extreme of our table are three tests which have an average

[1] *The Will-Temperament and its Testing*, p. 130.
[2] *Op. cit.* p. 71.
[3] *Op. cit.* p. 127.

negative correlation with one another and are, in general, negatively correlated with the tests in our first group. The main departure from the general negative correlation is the positive correlation between Tests 4 and 6 which has already been accounted for by an examination of the test activities; Test 6 is a test of non-suggestibility, and we should expect the non-suggestible type of person to score well in a speed of decision test of this nature. The suggestion is made that these tests reveal an opposite tendency to that revealed by our first group of tests. The first group of tests, to adopt Downey's titles, may be called tests of speed and fluidity (though Downey does not apply the titles to all these particular tests) and similarly the second group of tests may be regarded as tests of slowness and rigidity. The negative inter-correlations between the three tests in the group are not altogether unexpected as the tests themselves, it is suggested, throw light upon the different aspects of the slow, rigid, dogged type of person who would score high in these tests, the non-suggestible type given to "an intellectual balancing of reasons" in Test 6, the inhibited, somewhat obstinate, careful type in Test 9, and the persistent perseverating type in Test 12.

On the assumption that an approximate measure of the correlation of each test with our general factor is given by its correlation with the hypothetical general factor, a rough approximation of the specific correlations existing among our tests may be calculated[1]. Owing to the existence of group factors as well as a general factor in our variables, the figures in the following table cannot be accepted as an absolutely accurate measure of the relationships existing[2]. The figures in the following table may be used in support of suggestions made elsewhere in the paper, but, in view of the reservations stated above, which apply more definitely in the case of a table such as ours, where the presence of the general factor is not at first glance very obvious, no conclusions will be drawn from them.

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[1] For method of calculation see Burt, *The Distribution and Relations of Educational Abilities*, p. 53.

[2] Professor Burt, whose method is here followed, points out (in a letter to the writer) that it can "only be regarded as an approximation. As here applied it first assumes that the main trend of the correlations is determined by some general factor alone, and that any deviations will in the long run be neutralised as though they were due solely to chance. Having calculated the theoretical correlation with the hypothetical general factor, the method then proceeds to calculate the amount of specific correlation in isolated instances. For preliminary studies this seems to be the best procedure to use; but at the same time it can only yield a first approximation. It serves to indicate the presence of specific correlations, rather than to demonstrate their validity or measure their amount." I desire to express my indebtedness to Professor Burt for his very valuable comments upon the results of this investigation, and for his kind permission to quote from his letter.

Table V. *Specific correlations.*

	1	2	3	4	5	6	7	8	9	10	11	12
1	—	·463	·123	-·230	·138	-·162	-·025	·045	-·009	-·146	·059	·024
2	·463	—	-·071	·151	·115	-·071	·083	·120	-·067	-·281	-·081	·017
3	·123	-·071	—	·126	-·150	·005	-·195	-·021	-·180	·419	·028	·071
4	-·230	·151	·126	—	-·317	·138	·004	·042	-·161	-·029	·135	·093
5	·138	·115	-·150	-·317	—	·156	-·034	-·001	·012	·036	·030	-·023
6	-·162	-·071	·005	·138	·156	—	·075	·183	-·135	·012	·065	-·344
7	-·025	·083	-·195	·004	-·034	·075	—	·004	- 104	-·094	·146	·143
8	·045	·120	-·021	·042	-·001	·183	·004	—	202	-·198	-·357	-·033
9	-·009	-·067	-·180	-·161	·012	-·135	-·104	·202	—	·113	-·111	·001
10	-·146	-·281	419	-·029	·036	·012	-·094	-·198	·113	—	·144	-·035
11	·059	-·081	028	·135	·030	·065	·146	-·357	-·111	·144	—	·009
12	·024	·017	·071	·093	-·023	-·344	·143	-·033	·001	-·035	·009	—

The specific correlations, as here calculated, introduce some negative coefficients into our group of five tests, but it is doubtful if they are large enough to have any significance and may be due to some extent to the possible over-allowance for the general factor in the method of calculation adopted. If we apply the tetrad difference method to our group, whether we use the observed or the specific coefficients, only three of the fifteen tetrad differences are in the region of ± 3 or 4 P.E. and thus large enough to have significance, and the three cases on examination are found to be tetrads into which $r_{1\cdot2}$ enters, while $r_{10\cdot11}$ also enters into two of the three. Both of these coefficients, as we see from Table V, arise from tests which have high specific correlation, probably due, as we have suggested, to similarities in the test procedure in each of the two cases.

We have in our data, therefore, a group of five tests which, by their inter-correlations, reveal the presence of a group factor, and a second group of three tests which, although unexpectedly negatively correlated amongst themselves, appear to introduce factors which are negatively related to the group factor functioning in our first group of tests. We may now proceed to examine the nature of these factors in more detail and endeavour to relate them to general psychological theory.

5. THE NATURE OF THE GROUP FACTORS, AND THEIR RELATIONSHIP TO PSYCHOLOGICAL THEORY.

The results presented above apparently indicate the existence of group factors in some of the Temperament Tests under investigation; it is a more difficult matter, however, to identify the group factor in question and define it in psychological terms with any degree of precision. It follows from the nature of an investigation conducted with boys of school age that we have no introspective analysis by the subjects undergoing the tests to aid us in our efforts to interpret the results; we must, therefore, endeavour to throw some light upon the nature of the group factor by an analysis of the activities called into play in the tests themselves and the information to be derived from the correlation coefficients.

Let us consider first the group of tests which has been selected as indicating the presence of a group factor—Tests 2, 4, 11, 10, and 1, Freedom from Load, Speed of Decision, Co-ordination of Impulses, Interest in Detail, and Speed of Movement. Some indication of what is probably the most characteristic feature of this group factor may be secured by calculating the correlation of each of these tests with the pool of the remaining four tests. Using the coefficients from our table of correlations we obtain the following results[1]:

Table VI.

r_{t2}	and pool of	rest	·573
r_{t11}	,,	,,	·532
r_{t4}	,,	,,	·472
r_{t1}	,,	,,	·405
r_{t10}	,,	,,	·307

From these figures we may assume that the particular temperamental quality we are endeavouring to trace is most evident in the functions involved in the tests for Freedom from Load and Co-ordination of Impulses. High scores in these two tests may at first sight be regarded as the result of organization and co-ordination of native impulses, and on this assumption the prominence in our group of these two tests may be interpreted as evidence that our group factor rests upon the co-ordination and organization of native tendencies. The method of scoring the Co-ordination of Impulses Test, it may be said, appears to emphasize the speed factor, but a subject who is merely speedy does not necessarily score high, indicating that speed alone does not characterize the quality involved. Speed is only one aspect of the reaction resulting from the organization which, in the light of these two tests, may be regarded as constituting the basis of our group factor. The bringing of impulses and reactions under control by the development of the desired co-ordinations increases the apparent amount of nervous or emotional energy by conserving it, and this increased energy may express itself in terms of speed. On the physiological side this quality may find its correlate in the functioning of the nervous system. The structures which primarily determine function on a lower level secure a higher degree of efficiency and a wider range of adaptability through co-ordination with structures on a higher

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[1] The method employed was

$$r_{(a_1 - a_2 \dots a_{p-1})\, a_p} = \frac{S_p}{\sqrt{p - 1 - S - 2S_p}}.$$

See Ll. Wynn Jones, "A Note on the Correlation of Sums," this *Journal*, July 1924, xv. 21.

anatomical plane of the nervous system. "No fresh energy is generated by this higher integration, but the response gains in freedom; it can be more closely regulated according to the needs of the moment and brought into harmony with the reactions of the organism as a whole[1]."

This interpretation in terms of psycho-neural organization may be suggested as in close agreement with the functioning of Co-ordination of Impulses and Freedom from Load in the two tests bearing these names, and as accounting for the apparent emphasis upon the speed factor which is to be understood as increased efficiency of response and not as an increase in nervous or emotional energy. The explanation upon an *a priori* hypothesis of structure and organization is not in accord with the whole of our data, as we see when we examine more closely the inter-relationship of all the tests forming our group. The central place in the group occupied by the Co-ordination of Impulses Test is clearly due to its close relationship to the definite speed tests 1 and 4, with which it has coefficients of correlation of 0·308 and 0·435. Similarly, Freedom from Load owes its position in our table to its close relationship with the definite speed tests with which it has coefficients of correlation of 0·614 and 0·486; these four coefficients include the three highest in the whole table and there are only two other coefficients which slightly exceed the fourth.

Test 2 is purely a measure of the ability to maintain speed without external pressure, the ability to keep the psychic machine running speedily and smoothly. It is significant also that the highest coefficient in our table is that between Tests 1 and 2 which are purely tests of speed in writing in its speediest or natural form. The next highest coefficient, that between Tests 2 and 4, results from tests in which speed is definitely the chief consideration. The tests which stand just outside our group of five are also closely correlated with speed. Test 8, Finality of Judgment, has a distinct element of speed inasmuch as finality of judgment is dependent upon swiftness of decision, a suggestion which is borne out by the high correlation of this test with Test 4, Speed of Decision. Test 5 indicates energy and impetuosity of reaction which again is closely related to speed of reaction, and this test also correlates with the two main speed tests. One can only conclude, therefore, that speed is a general factor operating in all the tests in our group. Speed is but one aspect of a stream of energy which may vary in volume and rate of flow. The speed of a reaction is determined by the amount of emotional or temperamental energy, the greater the amount of energy the greater the speed. We may best describe our group factor, therefore, as the amount of temperamental or emotional energy which is available as a result of the instinctive drives, and the speed with which that energy expresses itself.

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[1] H. Head, "Speech and Localization," *Brain*, 1923, p. 425.

A reference to the table of specific correlation on the whole supports our conclusion, though we find negative correlation between Tests 1 and 4. This might be expected inasmuch as Test 1 measures speed of normal writing while Test 4 does not involve writing in any form but measures the speed in assessing one's own character traits. The specific table also suggests how close is the affinity between Test 5 and our group factor. This again supports our hypothesis, for the Motor Impulsion functioning in this test is measured by the ratio of speed and size of writing under normal conditions and under distraction. This is essentially a speed quality, as rate of writing together with size is the criterion, and large handwriting is usually also swift handwriting.

Our second group consists of tests which measure qualities which, in different ways, appear to function as inhibitors of speed and fluidity arising as a result of our group factor. Test 12, Volitional Perseveration, is a test the name of which is least indicative of the quality it is testing. The term volition as here used is not to be understood in the usual sense as resulting from persistence of ideas or stimuli. Careful study of the test in practice tends to confirm the view of the author of the test that it measures some aspect of 'psychic momentum.' There is no objectively established end for the task involved in the test, which is probably tapping a real perseverative tendency. The perseverator is one in whom the speed of flow of energy is slowed down; thus Lankes found[1] "that the perseverator on the whole tends to be slower than the non-perseverator." We should expect a perseverating person to be handicapped in a speed test, and to this factor we should attribute the "semblance of slowness" which Bernstein says[2] was given by "temperamental peculiarities." In Test 9 Motor Inhibition is measured by the extent to which the subject is able voluntarily to retard the time taken to write a given phrase. This is purely a conscious effort to check the speed of discharge of energy measured particularly by Tests 1, 2 and 4, with which it has negative correlation. This quality may be explained as the power to check the discharge of one impulse through blocking by the enforcement of another impulse. Suggestibility, measured by Test 6, Downey has found to be "interwoven with motor expansiveness or impulsion[3]," and, therefore, the suggestible person would be likely to excel in our speed tests: the non-suggestible, who score high in this test, we should expect to be more rigid and slow in reaction.

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[1] W. Lankes, "Perseveration," this *Journal*, 1915, VII.

[2] *B. J. Psych.* Mon. Suppl. 1924, VII, 21.

[3] *Loc. cit.* p. 115.

The negative correlation between these three tests is due to the fact that they represent different aspects of a process of inhibition which hinders nervous energy expressing itself immediately in motor reaction. Our definite negative correlation between Tests 6 and 12 is supported by the only investigation carried out up to the present which has any bearing on this question. Aveling and Hargreaves found[1] no tendency for suggestibility to be correlated with perseveration. Although negatively intercorrelated it is suggested that the qualities involved in these tests may function as a unitary trait however complex and varied it may be in origin.

The best psychological explanation of the results of our analysis of the data obtained in this investigation is on the general lines of Burt's theory[2] of general emotionality. This theory is best stated in his own words. "If the meagre evidence can be trusted, there seems to be, on the temperamental side as on the intellectual, a central or general factor. This may be termed 'general emotionality.' In addition, there appear to be at least two fairly broad special or group factors making the one for a repressed disposition, and the other for an unrepressed[3]." We have indicated above the existence of a general factor running through our tests of temperament qualities and this we suggest identifying with general emotionality. Our two group factors are best explained as due respectively to the predominant strength of the 'sthenic' or aggressive instincts (anger, self-assertiveness, curiosity, sex, and the like) producing an unrepressed temperament, and of the inhibitive group of instincts (fear, disgust, submissive tenderness, and the like) producing a repressed temperament. We thus reach the conception of two temperamental sub-types—"(i) the aggressive, and (ii) the inhibited respectively[4]."

This interpretation is in agreement with modern psychological theory. McDougall holds that "the essence of instinctive activity seems to be liberation and direction of energy[5]," and suggests that the most satisfactory position with reference to the fund of energy assumption is a combination of the two theories, one which postulates a common fund of energy for all the instincts, and the other which assumes the existence of a non-interchangeable amount of energy attached to each instinct.

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[1] "Suggestibility with and without Prestige in Children," this *Journal*, 1921, XII.

[2] *Brit. Assn. Report*, 1915, Section I, pp. 694–7.

[3] C. Burt, "The Measurement of Mental Capacities" (*loc. cit. sup.*).

[4] *Idem*, "The Definition and Diagnosis of Moral Imbecility," *Brit. Journ. Med. Psychol.* 1926, pp. 30–3.

[5] *An Outline of Psychology*, 1923, p. 107.

This provides a theoretical foundation for our conclusions regarding a general factor and group factors.

The classification has some features in common with Trotter's pair of contrasted types, the stable minded and the unstable minded[1]. The former type is energetic and active with "well-defined conative channels allowing of the strong and smooth outflow of psychic energy[2]" corresponding with our unrepressed temperament; the latter type is lacking in psychic energy or fails for some reason to give the energy effective expression thus corresponding to our repressed temperament.

The two group factors, it has been suggested, are related to the traditional distinction between two contrasted types, recently revised and popularized by Binet and by Jung. Jung's psychological types[3] are determined by the direction which the psychic energy or libido tends to take spontaneously; in the extravert (or objective type, as Binet termed it) the psychic energy is directed outwards, while in the introvert (or subjective type) it is turned inwards. The former type corresponds to our unrepressed temperament in which there is a free and unimpeded flow of nervous energy; while in the latter type the energy fails to find its natural outlets or is inhibited in some way, as in the case of our repressed temperament.

An interesting question arises when we compare the results of this investigation with the work of Webb[4] in which he puts forward the hypothesis of a general factor 'W' in character qualities, which is "in some close relation to persistence of motives." This general inhibitory factor is commonly regarded as positive in nature. Among recent writers Roback, for example, who regards inhibition as the basis of character, writes "Inhibition is thought to be a negative process. In reality, however, it is a positive force[5]." If this hypothesis were true our repressed temperament should have revealed the presence of a positive group factor and our table of observed correlations should be reversed. We can only conclude that our results fail to confirm the assumption that inhibition is a positive process. Professor Burt puts the matter very clearly in a letter to the writer. "I see in this result some confirmation of my theory of general emotionality and of two subordinate groupings of the specific instincts. Most earlier writers have been tempted to describe the emotional person as suffering from a lack of inhibition or control. They have taken inhibition as a positive factor. To me the emotionality is a positive factor; inhibition I regard as a negative process, the secondary result of many possible factors, different in different cases. If there were a central factor of inhibition, then we would expect the latter group of temperamental tests to reveal a general factor. On my theory, however, emotions may be slowed down or inhibited, in various ways—by each other, or by higher sentiments, or by acquired habits; their 'fluidity' may thus be impeded by a number of different factors which are not necessarily related amongst themselves. This seems confirmed by the presence of a negative rather than a positive inter-correlation among tests 6, 9 and 12 in your series[1]."

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[1] W. Trotter, *Instincts of the Herd in Peace and War*, 1923: "They (the unstable) tend to be weak in energy.... Thus we see society cleft by the instinctive qualities of its members into two great classes" (p. 59), etc.

[2] A. G. Tansley, *The New Psychology*, 1922, p. 104.

[3] C. G. Jung, *Psychological Types*, 1923: "Extraversion means an outward turning of the libido" (p. 542). In the introvert "the disposable energy is withdrawn into the self" (p. 145).

[4] E. Webb, *Character and Intelligence*, *B. J. Psych.* Mon. Suppl. I, p. 60.

[5] A. A. Roback, *The Psychology of Character*, 1927, p. 455.

In view of the relationship of our two types of temperament to the introverted and extraverted types it is of interest to quote McDougall's view that "It is uncertain whether introversion or extraversion should be considered a positive quality[2]." This appears to leave the question under consideration a problem that awaits solution. Tansley, however, writes: "The process of extraversion is the primitive biological function of the mind," which appears to imply that extraversion is a positive quality, and is in general agreement with the view formulated as a result of this investigation.

Within the limits of validity of this investigation we have identified a general factor and group factors in temperamental qualities. Further investigation of the problems underlying these conclusions is necessary to determine whether our factors cover all activities or whether they function only in the activities called forth by the stimuli presented in the conditions set down in the investigation. A recent writer who has analysed the conclusions of a number of psychologists who have studied temperament, while failing to find any universally admitted factor in temperament, ends her theoretical analysis of their work with the conclusion that "further efforts should be conducted on the lines of investigating differences in speed of reaction and in duration of maintenance of the intital level of activity[3]." The results of this investigation indicate the need for work in a somewhat similar direction, and emphasize the importance of relating the work to the instincts, which McDougall has termed "the prime movers of all human activity...and in them we are confronted with the central mystery of life, mind and will[1]."

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[1] (In a letter to the writer.) For a discussion of inhibition as a central factor see Burt, *Brit. Journ. Med. Psychol.* VI (1), 18–21.

[2] *Loc. cit.* p. 358.

[3] C. Bloor, *Temperament*, 1928, p. 152.

The conclusions reached in this investigation are necessarily tentative, as sufficient evidence is not available at present on the subject to warrant any finality or definiteness of statement. The tests used are certainly bringing to light innate qualities and the group factors isolated apparently have significance. Working on these lines we may hope to throw some light upon the problem of Temperament in the solution of which so little of definite value has yet been accomplished. The need is clear "for the problems of temperament and character constitute one of the most difficult and urgent provinces for future psychological research[2]."

6. SUMMARY AND CONCLUSIONS.

We may summarize our results by stating briefly the conclusions which, apparently, may be drawn from the investigation:

(1) There is a general temperamental or emotional factor entering into the T tests which is closely related to general emotionality and may provisionally be identified with it.

(2) There is little if any direct relationship between temperament as measured by these tests and intelligence.

(3) (*a*) Examinations and intelligence correlate on account of a general intellectual factor which we identify with Spearman's '*g*.'

(*b*) Examinations and temperament correlate on account of a general temperamental or emotional factor. This will account for the failure of some boys of high intelligence to do well in school examinations.

(4) There are apparently two group factors operating in the qualities involved in the T tests.

(*a*) One group factor manifests itself in the speed and ease with which temperamental or emotional energy is allowed to express itself.

(*b*) The other group factor functions as an inhibitor of the speed and fluidity with which the energy can find vent.

(5) These two factors tend to yield an unrepressed 'type' or temperament and a repressed 'type' or temperament, determined, it is suggested, by the relative predominance of the aggressive group of instincts and the inhibitive group of instincts respectively.

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[1] *Social Psychology*, 13th ed. p. 44.

[2] Consultative Committee—Report on "Psychological Tests of Educable Capacity," p. 60.

(6) Emotionality, it is suggested, is the positive factor, and inhibition a negative process resulting from several factors which are not necessarily related amongst themselves.

(7) Our general temperamental or emotional factor is identified with the emotional energy underlying all the instincts, and our group factors are determined by the subordinate groupings of the instincts with their specific energies.

The investigation, therefore, confirms Burt's theory of general emotionality with sub-factors due to the aggressive and inhibitive instincts, and is in agreement with McDougall's view of the common fund of instinctive energy and non-interchangeable energy attached to each instinct.

(*Manuscript received* 4 *March*, 1929.)

NOTES ON THE EMOTION OF FEAR AS OBSERVED IN CONDITIONS OF WARFARE

By F. AVELING.

(*From the Psychological Laboratory, King's College, University of London.*)

1. *Sources of information* (pp. 137–138).
2. *The nature of fear* (pp. 138–139).
3. *The antecedents and conditions of fear* (p. 139).
4. *The immediate effects of fear* (pp. 140–141).
5. *The after-effects of fear* (p. 141).
6. *The control of fear* (pp. 141–144.)

THE following 'Notes' were compiled at the request of a colleague for a collection of psychological observations with relation to military activities. They were written at La Lovie in the summer of 1917; and only recently found their way back to the hands of the author, with a suggestion that they might be worth publishing. They are accordingly offered, with some academic diffidence, almost exactly in the form in which they were originally put together, such slight modifications only being made as seemed desirable in order to adapt them to that of an article in the *Journal.*

1. SOURCES OF INFORMATION.

With the exception of a certain number of cases suffering from the after-effects of extreme terror (possibly also complicated with true shell-shock) which he kept for a short time under observation, the writer was able to undertake no experimental research in which exact measurements could be made and from which statistics could be derived. He is able, therefore, merely to advance a certain number of generalities, based upon personal observation, introspection and retrospection, as well as upon a number of relations of officers and other ranks whom he questioned with regard to the subject. Although there is much omission, for the purpose of this paper a certain amount of psychological commonplace has been included; but at the same time there are certain observations which appear to be new, or at any rate newly emphasized in the conditions of actual warfare. Occasions for the eruption of fear were certainly not lacking; and opportunities for the exercise of self-control were as frequent as were the occasions tending to disequilibrate emotional stability. The descriptions given to the present writer by those whom he consulted, whether in the relatively fixed units such as Army Corps or Headquarters, or in Rest Camps or Casualty Clearing Stations, as to the experience of fear itself, the character of the mental processes both emotional and intellectual under the influence of fear, and the effect of other spontaneously arising or induced mental factors upon fear and self-control, were remarkably unanimous, allowance being made for individual differences of mental temperament, for the physical circumstances in which the fear complex arises, and for the physiological state of the subject at the time.

2. THE NATURE OF FEAR (DESCRIPTIVE).

1. Fear is a simple, instinctive emotion, impossible to define or directly analyse. It is susceptible of degrees, and is most easily characterized by its psychical and physiological effects. (*Cf. infra*, Section 4.)

2. As an emotional state, it tends to pervade the entire field of consciousness, whether it be weak (nervousness or apprehension), strong (fear), or dominant (terror).

3. It has no contrary emotion, with which it could be represented graphically by a single line with an 'indifference point'; but it can be represented by a line originating at zero and rising to a point at which complete mental disequilibrium (unconsciousness, insanity, etc.) occurs.

4. Together with the course of the emotion of fear, there is present in consciousness an evolution of cognitional processes (percepts, ideational and imaginal contents, etc.) which apparently act upon and are reacted upon by the emotional course. (*Cf. infra*, Section 4 (*b*).)

5. There are also generally present to consciousness feelings of psychical strain or tension, unpleasantness and excitement; as well as bodily sensations, especially muscular (notably heart), visceral and respiratory, together with sensations from and consequent upon changes in secreting glands (salivary, sweat, etc.).

6. Many, if not all, of these feelings and sensations can be discovered by introspection during the course of development of a fear crisis. They can generally be retrospected and recognized with comparative ease by any observer, trained or untrained.

7. The eruption of fear in consciousness is usually, though not invariably, abrupt; though the course of its development from weak to strong, or *vice versa*, may be observably of very considerable duration. The termination of the course may be gradual or abrupt.

8. Besides being highly unpleasant and distressing, fear as a mental state and on account of its physiological repercussion unfits those who suffer it, to a greater or less extent, for the performance of whatever task or duty they may have in hand. The more or less complete disequilibrium of fear varies inversely with self-control.

3. THE ANTECEDENTS AND CONDITIONS OF FEAR.

1. Fear has as its cause (antecedent) an objective content of consciousness either perceptual, imaginative or ideational. With this is connected either the consciousness of a relation, or a judgment, or belief that the object or situation involved is formidable (threatens in some way the subject's well-being).

2. There would seem to be enormous individual differences in the matter of susceptibility to fear, varying from almost complete fearlessness to extreme predisposition to terror and panic. It is obviously impossible to compare or contrast fear in different individuals with regard either to its quality or quantity. At most, judgments could be made on the grounds of comparison of bodily actions caused by fear; and such judgments would be utterly untrustworthy on account of other disturbing factors (as self-control, etc.) intervening. In different experiences of fear, however, on the part of the same individual comparisons and contrasts can be made; and subjects consulted bore the present writer out in considering certain conditions as predisposing to fear. Here again factors other than those to be noted may have their effect on the fear arising in any given situation, emphasizing or neutralizing the influence of these predisposing conditions. Nevertheless, a certain number out of what in all probability is a very large number of them may be noted; as, for example, (*a*) *Psychical Antecedents:* morose brooding on thoughts of fear; evil conscience; awareness of fear in others; sense of isolation; psychical fatigue; insomnia (caused, *e.g.* in warfare by sound of guns, etc.); unfamiliarity of situation; associations between things or places with previously experienced fear; (*b*) *Physiological Antecedents:* general state of health, lowered vitality, etc.; hunger (up to a certain point only); muscular fatigue (also only to a certain point); indulgence in alcohol, sexual abuse, etc.; lack of physical occupation, idleness, etc.; (*c*) *Physical Antecedents:* asthenic conditions, as dull, bad weather; darkness (night); unfamiliarity of place; evidences of previous formidable activity (often offset by curiosity, etc.).

4. THE IMMEDIATE EFFECTS OF FEAR.

Whatever view may be taken as to the relation of the emotion of fear to the physiological manifestations of it, it would seem to be indubitable that the bodily changes which occur have an extremely large part to play, at least in intensifying the emotion. The great mass of feelings and sensations, to some of which reference was made in Section 2, note 5, come streaming into consciousness, some of them more or less distinct, others vague and obscure. Many of these sensations are unusual, or unusually intense. Their simultaneous presence in normal life is exceptionally rare. The great amount of nervous energy that is liberated has its psychical counterpart in the mental energy liberated and the psychical changes that take place. Of these the intensifying of the emotion is one, whether because of the liberation of energy upon which it feeds, energy which sustains and increases it; or whether the actual bodily state is in itself formidable. However this may be (the writer is inclined to think that both may be true) the following are some of the phenomena which should be classed under the heading of "Effects of Fear."

(*a*) *Physiological effects* (introspected). These are well known. Included among them are: phenomena of attention; innervation of the eye-muscles, with fixation of the eyes; of the neck and shoulder muscles, with rigidity of posture; of other skeletal muscles governing posture (introspectively the two first named are the most pronounced). When trembling, etc., mentioned below, occur involuntarily, there is generally a conscious effort to innervate these muscles in order to secure rigidity. There is often a quick retardation of respiration, followed by a rapid and shallow breathing. The heart-beat seems to stop, and then to be much accelerated. Changes in the secretion of certain glands occur. Most noticeable among these are the salivary (retarded or arrested); sweat (palms of hands, forehead, etc.); in some cases, lachrimatory. (These are all, as a rule, present to consciousness.) Changes take place in the sympathetic nervous system. There is relaxation of sphincters; general feeling of sinking in pit of stomach; desire to urinate, etc. Contraction and relaxation of skeletal muscles occur; trembling, teeth chattering, etc.; sometimes spasmodic choreic movements, cries, etc. Later, there is lassitude and fatigue.

(*b*) *Psychical Effects.* Feeling of helplessness (consequent on physiological effects of fear); feeling of tension ('tending toward'; a characteristic phenomenon of attention); feeling of aversion; narrowing of the field of attention, with (?) heightening of intensity of its contents;

quickened, or accentuated, processes of thoughts, judgments, etc.; in more extreme cases confusion of thoughts, judgments, etc.; enhanced vividness and liveliness of imagination; quickened judgments and decisions; or retarded judgments and decisions; vague mass of sensation, from which individual sensations may stand out with prominent emphasis; in extreme cases total loss of self-control, unconsciousness, insanity, etc.

5. THE AFTER-EFFECTS OF FEAR.

Certain of the effects of fear tend to persevere in some cases after the emotion as such has disappeared. There are extremely interesting cases in which one or more of the physiological manifestations (effects) as, for instance, tremor, local anaesthesias, aphasia, amnesia, etc., endure long after the formidable object, or circumstance, has ceased to exert any direct effect upon consciousness. These subjects, who present the ordinary symptoms of hysteria, are very susceptible to suggestion and, in the vast majority of cases, are easily cured of their trouble and its manifestations byit. It would appear that in such cases a 'fear complex' has become buried in the subconscious mind, of which the effects persist after the manner of effects of unconscious wishes. This is a subject which, in the opinion of the present writer, would well repay careful investigation. Doubtless many other internal secretions are stimulated, or retarded, as those of the thyroid, adrenal and hepatic glands (stimulation); and effects brought about such as hyperacidity of blood, and the like.

6. THE CONTROL OF FEAR.

Exact psychological information with regard to normal mental processes would suggest the employment of certain more or less obvious methods as useful in the maintenance of self-control in the presence of situations likely to induce fear, or in recovering it once disturbed by fear; and, as a matter of fact, several of such methods have been found by experience to be useful. Some of these methods are not of a purely psychological character, but are here included because of the indirect effect they produce upon the emotion itself. In the majority of the methods indicated more than one psychical mechanism is evidently employed, of which the chief would seem to be: direction of attention; substitution or suppression of motives; draining away of neural and psychical energy; transfer of psychical energy from one motive to another; alteration of the physiological effects of the emotion which tend to keep it in being and increase its intensity.

The chief methods indicated are the following:

1. Drugs, alcohol, etc. These may have a more or less immediate beneficial effect; but the subsequent effects are favourable to the genesis of fear.

2. Bodily activity. To be actively engaged upon some form of work helps in the suppression of fear and the maintenance of self-control. This is probably due to more than one factor. There is a draining off of the nervous energy through the channels of the nerves supplying the muscles involved in the performance of the work. There is also distraction of attention (cf. *infra*, 5). The process of draining off energy in this case may be compared to that of the curious tricky habits of some public speakers (twirling watch-chain, fiddling with eye-glasses or the button of the coat, etc.; of which it has been plausibly suggested that the draining of energy inhibits the ready emergence of irrelevant associated and free ideas, and thus assists a clear and logical sequence of thoughts connected with the topic dealt with).

3. Breathing, deep and regular. To a certain extent this method can be consciously employed with success. It would appear that such breathing has its repercussion on the circulatory and perhaps other systems, thus tending to counteract some of the unusual and irregular physiological activities of which mention has already been made as increasing the emotion (cf. *supra*, Section 3, 1).

4. Singing, especially chorus singing, aloud. The rhythm makes for good breathing. Attention is distracted. A sense of gregarious solidarity which tends to inhibit fear is set up. Probably also the tune and words of the song arouse associations which drain off energy from the emotion.

5. Distracted attention. This would seem to be one of the most efficacious methods of controlling fear. It is obviously, however, difficult to distract attention in circumstances occasioning strong emotions merely by voluntarily attempting to alter its direction; and this is especially true while the subject is experiencing the emotion of fear. Nevertheless, the desired effect may be brought about in various indirect ways, thus providing a motive for self-control. The subject may occupy himself with some manual work or physical labour. He may receive a command from a superior officer which he must obey, or concerning which he must think actively. Cases have been observed in which, under the influence of fear, an objective content or representation has arisen spontaneously or by association in consciousness, to which a new emotional tone has attached. The writer does not know of cases in which it has been entirely inhibited. Religious considerations which arise voluntarily or spontaneously in fear crises no doubt also bring this mechanism into play.

6. Feeling or sense of responsibility. This would apparently involve the mechanism of distracted attention, at the same time providing a powerful motive for self-control.

7. Human respect; shame of appearing to be afraid; possibly also (though the present writer does not possess very strong evidence for this) thoughts or representations of the penalties of giving way to fear and cowardice. Considerations of this nature bring the self-centred and self-seeking feelings into play. These are strong feelings; they constitute a strong motive, and involve draining and transfer.

8. Tranquil conscience. This may be secured in various ways, chiefly by religious and moral means.

9. Imagination and instinctive false judgment. The practical instinct to take cover manifests itself in greater confidence and decrease of fear even when cover is totally inadequate and worse than useless. Any imaginary cover is better than none so far as the emotion is concerned.

10. Emotional conflict. It is possible to decrease, and even totally to inhibit fear by securing its conflict with other emotions, as anger, hatred, etc. Fear may change abruptly to anger or hatred, following on the emergence in consciousness of a presentational or imaginative content, for which the latter, in the totality of the given circumstances, is an appropriate emotion. This may also be substituted for the former by the use of an intermediary idea implanted from without, as well as by even trivial irritating circumstances occurring during the experience of fear. But at the same time it must be noted that fear and anger, at any rate in their more feeble forms, can co-exist simultaneously in consciousness.

11. Suggestion. Predisposition to fear attacks is somewhat lessened by the use of suggestions given in the hypnoidal state. The writer has no data with regard to suggestions given in deep hypnosis. But, in the case of subjects readily disposed to fear, the duration of the persistence of hypnoidal suggestion seems to be limited; and fresh occasions of fear tend to break down easily the implanted 'self-control,' 'self-confidence,' 'strength of will,' etc., complexes. Simple suggestion during a fear crisis is often distinctly beneficial. The mere presence of others who are collected and calm helps subjects inclined to fear to preserve self-control. Sometimes a superstitious belief in the luck of visible and tangible objects, as mascots and charms (contagious magic) acts as a species of suggestion or auto-suggestion with appreciable results. Prestige, both professional and personal, is of large importance in the use of methods of suggestion.

12. With regard to the removal of mental and physiological effects

of the symptoms of fear (shock) which persist when the actual emotion is removed, these have been treated with remarkably good results by simple suggestion, suggestion with faradization, hypnoidal suggestion and hypnosis. Individual cases vary enormously; and the particular method best to be employed in each must be determined by the neurologist or psychologist. Tremors, local anaesthesias and sensory disturbances, etc., are generally amenable to the first three methods named; amnesia is most easily treated by the third and especially the fourth. Patients are exceedingly susceptible to suggestions of all kinds connected with their malady, or supposed malady; and the greatest care should be taken in diagnosis and treatment not to induce ideas and symptoms by injudicious questioning, manipulation, or in any other way which might lead the subject to imagine them.

(*Manuscript received* 14 *May*, 1929.)

SOME SUBTLER SKILLS

By T. H. PEAR.

(From the Department of Psychology, University of Manchester[1]*.)*

A definition of skill (pp. 145–146).
Skill and ability for deliberate, adequate, verbal expression (pp. 146–147).
Speech habits (pp. 147–149).
The prelude to skill (p. 149).
Anticipation and intuition (pp. 149–150).
Verbal descriptions of visceral experience (pp. 150–151).
A visualizer's attitude towards the 'verbalization' (sic) *of his thoughts* (pp. 152–153).
Really subtle skills (pp. 153–155).
Are we becoming more conscious? (pp. 155–156).
Skill, technique and decency (pp. 156–158).
The stylization of subtle skills (p. 158).
The fate of skills that are left alone (pp. 158–159).
Prevalent attitudes towards important skills (p. 159).
The social meaning of these considerations (pp. 159–160).

In recent publications[2] I have suggested an analysis of skill. From those treatments the subtler human skills were exempted. The present paper will sketch a tentative analysis of some of them.

A DEFINITION OF SKILL.

In the writings mentioned, skill was treated as behaviour and as experienced 'from inside.' It was defined as (*a*) the integration of well-adjusted responses, and (*b*) the kinaesthetic knowledge which is the immediate concomitant of such behaviour.

It is supremely important to distinguish between kinaesthetic, or stataesthetic, knowledge, obtained through the locomotor apparatus, and that which has arrived by way of the eyes, ears, tongue, nose, etc.[1] In practice, obviously, some persons may find it unnecessary to distinguish them. Yet this fact does not justify such a confusion in psychology. A person who has learnt by heart the best prescription for an omelette may not make one as well as an illiterate peasant. Applied science continually demands a distinction between these classes of knowledge; *e.g.* the 'eye-ear' knowledge of chemical processes which even a paralysed man might have, and the skill in causing certain chemical changes, which might be the pride of an 'ignorant' workman. In medicine the deliberate divergence of knowledge and skill is too well known to require emphasis. Yet in discussions of industrial psychology such a distinction still appears to surprise many practical persons.

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[1] Substance of a paper read to the British Psychological Society on 9th March, 1929.

[2] *Fitness For Work*, London, 1928, Chapters I, II and VI; "The Nature of Skill," Presidential Address to Section of Psychology, British Association for the Advancement of Science, Glasgow meeting, *British Association Addresses*, 1928; "The Nature and Present Position of Skill in Industry," *The Highway*, London, Feb. 1929.

SKILL AND ABILITY FOR DELIBERATE, ADEQUATE, VERBAL EXPRESSION.

I have already emphasized[2] that while in some people these two abilities co-exist in high degree, in some they do not, and in still others they appear to be almost inversely related. Obviously, correlations between these or any other abilities cannot be predicted without scientific investigation. But since, while some skills are richly endowed with expressive words, others are not, it is clear that, in relation to certain skills, few people will be found possessing both these abilities in a high degree.

Following a remark of Mr Bertrand Russell's in his paper on "The Meaning of Meaning[3]," I proposed that this ability for deliberate, adequate, verbal expression should be termed 'intellect.' The suggestion has not aroused enthusiasm. I am therefore desirous of finding a word to mark off this important ability. For important it is though nameless, and like some nameless people, by that fact, embarrassing.

The phrase 'speech habits' expresses exactly what this word must *not* mean. I do not propose to use for it a letter of the alphabet. To encourage this tendency runs the grave risk of a newer 'new psychology' with twenty-six faculties.

I should like to lay stress upon 'deliberate' and 'adequate' in the phrase at the head of this section. Having been asked whether, if an under-tipped taxi-driver fluently reviled his 'fare,' he would be using his intellect, my answer is that in and so far as he is using speech habits, he is not intellectual. But in and so far as he is deliberately collecting, discriminating and choosing his linguistic gems that they may precisely describe his attitude, he is intellectual. Or, of course, whatever better adjective may be found to describe this quality.

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[1] Cf. *Remembering and Forgetting*, London, 1922.

[2] *Fitness For Work*, Chapter II; *Highway*, *loc. cit.*

[3] *Mind*, N.S. vol. XXIX, Off-print No. 116.

SPEECH HABITS.

I should welcome a temperate use of the term 'speech habit,' to denote a specific response to a specific situation, bearing the other mark of habit: automaticity. Such a term would mark off from the immediate products of thinking so many expressions which, once the product of thinking[1], are now mere habits.

Speech, of the kind which enters into forensic skill, may consist of habits. They, however, may appear to the uninstructed listener to be fresh utterances. A hardened committee-man, opening his attack with "Unfortunately I do not possess the technical knowledge...," occasionally means just that. Often he implies, "I have, however, the robust common-sense which is better than the expert's crankiness," or, "I have the penetrating intelligence which makes technical knowledge superfluous." He may even emit these habitual phrases in a bored way, as a fencer may languidly parry the strokes of a weak opponent.

While complicated modes of speech may be habitual, simple modes of speech obviously need not be habit.

The spectator of a skill may care little if it be habit or a novel production. For a psychological observer the difference is supremely important. Even the statement that Art is to conceal art—a supremely skilful series of words—warns against the simple-minded muddling of skill with habit. Many pupils of a teacher of skill whom I know display an apparent careless 'natural' ease. That their teacher presents an unfailing example of this light touch is not without significance. What therefore appears to the spectator as a pattern of easy habits may be a technique, acquired by imitation, analysis, thought and assiduous practice. (One easy way to discover truths about this is—curiously enough—to ask the performer.)

The use of the term 'habit' by some behaviourists.

Recently some American psychologists Divorced Old Barren Instinct from their Bed. Their writings now suggest that they Took the Daughter of the Vine to Spouse. For what they—their critics too—would call their speech habits have rapidly regressed from epicritic to protopathic. There is a lack of discrimination between blatantly different things, and a coarsening in graduation of response. This is not all. It is a tenet of science, and not less of the Victorian Fundamentalist physical science to which some behaviourists appeal, that having discriminated things one should henceforth call them by different names. Yet when the word 'habit' is made to stand for—or eclipse—sensory discrimination, will, ability, art, intelligent adaptation, routine adaptation, independent judgment, inventive imitation, moral behaviour, thought, sensitive perception, flexible response, and even the self, the psychologist's world becomes a fog, in which, notoriously, things appear alike.

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[1] For a discussion of the nature of thinking see Henry Head, *Aphasia and Kindred Disorders of Speech*, London; F. C. Bartlett, "The Relevance of Visual Imagery to the Process of Thinking," this *Journal*, 1927, XVIII, 23; T. H. Pear, "Is Thinking merely the Action of Language Mechanisms?" this *Journal*, 1920, XI, 71.

It is sadly true that the meanings of words get worn with use. But the quickest way to wear the edge off any word is to use it for many different things. This habit, attributed in our own country to the schoolgirl, should be discouraged in adult psychologists writing in any language.

Subtle skills.

Most of the skills I have taken as examples in previous writings have been relatively unsubtle. Skills which we like watching in others usually lend themselves to 'behaviouristic' study. Most of them involve chiefly the larger muscles, though striking exceptions in some countries are speech and song. Is there, however, difference of *kind* or only of degree, between simple and subtle skills?

I believe that the difference is one of degree; yet, since the chief components of the subtler skills are shyer and more elusive, they are apt to be avoided or passed over by the practitioners of the behaviouristic method.

I may perhaps illustrate this by taking as an example the technique aimed at in the special art of the 'radio talk.'

To prepare and to give one, as distinguished from merely reading passages composed in written style—which may be magnificent but are not a 'talk'—is to aim at a new set of skills. The technique requires collection of possible, followed by choice of suitable, matter. Such choice demands discrimination with an aim in view. This aim may be difficult to fix upon, since the composition of the audience is a matter for speculation. There must be consideration and arrangement of the order in which the points are to be presented, and of the relative stress, psychological, aesthetic and acoustic, which they will receive. Since, at the moment of delivery, moods and 'overtones' of meaning can be conveyed by vocal skill alone, the expert talker may mentally rehearse his intonation, even marking his manuscript accordingly. That special speech-device made possible by the microphone, the trailing-off of the voice, like the 'fade-out' on the screen or the dimming of a stage; the hesitations, even the words found after the hesitation, may be deliberately planned and rehearsed. To describe it will seem to many as indecent as the uncovering of any other technique, a theme which will presently be discussed.

This technique therefore owes much to artifice, whatever that word when analysed may mean for psychology. This is also true of the subtle skills, shown by a lawyer or a hostess, foreseeing and avoiding possible *contretemps.*

THE PRELUDE TO SKILL.

I would suggest that it is profitable to consider anticipation, collection and selection, as the prelude to skill. For one who has acquired taste and discrimination in a purely verbal or bookish way, may be able to imagine how he would act skilfully and yet be clumsy in his actual dealings with others. Usually the extravert displays real social skill; the introvert doing it in his dreams. And skill in dreams appears to be an untouched subject.

In art and sport these distinctions are clear and accepted. Wimbledon attracts many persons, whose taste and discrimination permit them to appreciate the fine points of a tennis stroke which they will never make. Some stage dancers, who possess great skill, fortunately rely for taste and discrimination upon their artistic directors.

The first part of our definition of skill, the "integration of well-adjusted responses," might be accepted by a behaviourist, though the word 'integration' may suggest trouble for him. Less convenient for him is the second part, implying that skill involves power and *knowledge.* For an essential in many skills is fore-knowledge or anticipation.

ANTICIPATION AND INTUITION.

Skill and anticipation.

The relation of skill to anticipation raises a problem of great theoretical significance. High-grade skill, whether in a motorist or in a lawyer, implies ability not only to avoid actual obstacles, but to 'foresee' them. I fence this word by inverted commas to make it clear that that seeing, actually or in imagination, is not the only method of anticipation.

By what processes are obstacles anticipated? In some persons verbalizing seems pre-eminent. But, when one is drafting the presentation of a proposal so that it shall meet with fewest objections from a committee, visual imagery of the expressions or actions of the members may play an important part. In some of us, as I have urged elsewhere, it is too insistent and ubiquitous to be denied significance, or relevance[1].

Skill and intuition.

The use of intuition as an explanatory principle has diminished lately. A possible reason, apart from the term's vagueness, is the rapid incursion into our daily life of mechanical inventions. These present problems to solve which scientific information is indispensable. A person may intuitively judge that a child, who, a moment ago, was crying merely to attract attention, is now expressing pain. But no such gift will tell him the cause of microphonic noises or 'motor-boating' in his wireless set.

By some, the term 'intuition' is restricted to signify an adequate unlearned interpretation of a biologically important situation, the essential features of which have often occurred in the history of the race, though not in that of the individual. This use of the term would raise all the intricacies implied in the first part of Professor W. McDougall's definition of instinct, "an innate psycho-physical tendency to *perceive*... objects of a certain class[2]," and all the problems of *Gestalt*.

The question whether man possesses any inherited skill is difficult. At birth he is unskilled. But the absence of skill in very early life may be due in part to the necessity for appropriate maturation[3] of the bodily apparatus. Experience is also necessary to form the setting, the background and the provocation for skill.

VERBAL DESCRIPTIONS OF VISCERAL EXPERIENCE.

To my mind the most interesting and helpful of the behaviourist's conceptions, invoked to explain thinking, is that as education progresses visceral experiences may become 'verbalized.' (We enter a *caveat* here against this use of the word 'verbalize,' returning to the subject on p. 152.) Many psychological explanations which nowadays involve an 'unconscious,' may be possible in terms of visceral and muscular experiences to which their possessor has given no names. Accordingly, they have not been focussed clearly against their unanalysed background[4].

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[1] "The Relevance of Visual Imagery to the Process of Thinking," this *Journal*, 1927, XVIII.

[2] *Introduction to Social Psychology*, London.

[3] Cf. K. Koffka, *The Growth of the Mind*, London.

[4] This should not allow us to forget that many insistent intrusions of the 'unconscious' take the form not of muscular and organic sensations, but of visual, auditory and other images (cf. *Remembering and Forgetting*). This fact will not be forgotten by doctors, artists and writers, but is apt to be ignored by behaviourists.

Such 'verbalizing' of visceral experience may play two important parts, both in the exhibition and the inward realization of the subtle skills.

(1) Certain techniques which are nowadays supposed to be, in great part, 'unconscious,' might be made more conscious, or even completely conscious. (This idea will be developed on p. 155.)

(2) By being brought to consciousness, the unverbalized organic and muscular experiences which now add to certain skills an undertone of emotion (sometimes weak, sometimes overwhelming) might lose their mystery. Their consequent 'subterranean' effect might then cease to hamper the skill.

From experience I know that not a few athletes who, in the games they play, have fallen many times, admittedly fear falling on ice. Their fear is often based upon wrong mechanical, anatomical and other ideas with their accompanying actions. It is decreased when the ideas are corrected, and the learners are shown how to fall, how not to, and why.

No psycho-therapist doubts that talking about certain unclear but insistent organic complexes and muscular attitudes may help one to become familiar with them; even sometimes to control them. Talking about a thing which one fears may either increase or decrease the fear, but seldom leaves it unchanged. *Mutatis mutandis*, this is probably true for all instincts, especially those connected with sex. Finding or inventing words for these unclear experiences, using them with meaning, and, uttering them so often that they become speech habits, may dull the edge of fear, lust, and more complex emotions. How, exactly, verbalization performs this liberative function is not known.

Expressing frankly in words, to oneself or another, unclear experiences which have stirred one up organically, seems to have an early and a late effect. At first, talking about 'censored' but dawning experience might increase the intensity of its concomitant organic disturbances. Examples of such experiences are those connected with fear, sex, or even, in some persons, acquisitiveness. Such disturbances are produced in some people by reading Mr Bertrand Russell's *Sceptical Essays* or Dr J. B. S. Haldane's *Daedalus*.

At a later stage, familiarity breeds not contempt but automatism. Mr Michael Arlen causes less excitement as time goes on. And when one discovers some of Mr D. H. Lawrence's formulae, his writings are less perturbing, though never perhaps really restful.

A VISUALIZER'S ATTITUDE TOWARDS THE 'VERBALIZATION' (*sic*) OF HIS THOUGHTS.

Professor James Dewey has recently told us that the novelist and dramatist are much more illuminating as well as more interesting commentators upon conduct than the schematizing psychologists[1]. A contribution of these latter towards the dullness of life is the crude, question-begging supposition that visual imagery can be *expressed* in words, as pounds can be expressed in francs. Yet does anyone, not already besodden with words, either believe or attach any real meaning to it? In modern non-academic writings we find this expressed better than in a shelf of textbooks on psychology, with that brilliant exception *Studies in Aphasia* by Sir Henry Head.

Miss Victoria Sackville-West writes, in *Passenger to Teheran*:

> It may be that language, that distorted labyrinthine universe, was never designed to replace or even to complete the much simpler functions of the eye. We look; and there is the image in its entirety, three-dimensional, instantaneous. Language follows, a tortoise competing with the velocity of light, and after five pages of print succeeds in reproducing but a fraction of the registered vision. It reminds one of the Oriental who with engaging naïvety thought that by photographing the muezzin he would record also the notes of his call to prayer. The most—but what a most!—that language can hope to achieve is suggestion; for the art of words is not an exact science. We do not indeed reflect often enough how strange a world-within-the-world we have created by this habit of language, so strongly rooted in us by tradition and custom, so taken for granted, that we are no longer capable of imagining life without it, as one of those ideas which the mind is unable to conceive, like the end of time or the infinity of space....
>
> Moreover, the contradictions contained within the capacities of language are violent and astonishing. At one moment it seems that there is nothing (within the limits of our experience) that may not be expressed in words, down to the finest hair-stroke of a Proust or a Henry James; next moment we recognize in despair, so poor is our self-imposed vehicle, our incapacity truly to communicate to one another the simplest experience of our factual or emotional life. Who amongst us could boast that, transplanted into the mind of another person, even though that person be his nearest, he would not find himself in a strange country, recognizing here and there a feature that he knew, but on the whole baffled by unexpected grouping, shape, and proportion? There is only one province of life with which language is almost fitted to deal; the province of the intellect, because that is the province, so to speak, begotten by language itself, which without language would never, could never, have come into existence. Those things which are felt, and those things which are seen, because they exist independently, and in no ratio to the degree of our articulateness, are not the business of words.

Mr Robert Graves in *The Meaning of Dreams* and *Poetic Unreason*, and Mr J. D. Beresford in the first pages of *Writing Aloud*, explicitly deny that in any way words can be the equivalent of the experiences accompanying visual imagery.

So, feeling that to surrender his intimate experiences for the worn counters of common exchange is to distort and cheapen them, the visualizer is unenthusiastic about the revelation to others of his private techniques, even if this be possible; which, I imagine, some cartoonists and caricaturists would doubt strenuously.

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[1] *Human Nature and Conduct*, London, p. 154.

I do not suggest that the verbalizer has no mental privacy[1]; that his meanings are entirely expressed by his words, that, so to speak, his whole mental life takes place on the front porch. Yet, according to some behaviourists, the verbalizer's processes are, in theory at least, open for all to observe; the intellectual, as Mr Bertrand Russell remarks, being merely an acrobat with his linguistic mechanisms.

A number of psychological problems lie rather inaccessibly in the fissure between visual imagery and its so-called verbal equivalents. At present, workers on both sides of the crevasse seem too busy to notice it, or its contents. Yet the fact that daily leaps are made across it by psychologists—to say nothing of writers upon language—must be recorded by any fair observer.

REALLY SUBTLE SKILLS.

My thesis will now, I hope, be plain; low-grade coarse skills fit fairly well into the behaviourist's programme of study; subtler ones do not. Yet a fair number of their problems can be squeezed in. For the main framework of the bodily expression, even of delicate adjustments involving social and forensic skill, may become habitual; *i.e.* an order of acquired specific responses. 'Skill' may usefully name the executive aspect of the performance. But skill also has a specifically psychological component; kinaesthetic knowledge. This is the awareness of the acts, either as they are carried out, or as they are not carried out, for inhibition is a positive occurrence. This awareness will present cognitive, affective and conative aspects, the relative aggressiveness of which will differ at different moments.

What is to be said of the skill in anticipating, by means of kinaesthetic sensations? So far as they function in this way they are the experiential counterpart of the externally exhibited behaviour. But anticipation by the use of non-kinaesthetic, including many non-verbal images, includes myriads of everyday occurrences which decline even to approach this explanatory bed of Procrustes. Some of them, especially the visual images, annoy simple-minded behaviourists as the *ignis fatuus* got on the nerves of early scientists. Ford Madox Brown has depicted John Dalton, prone above a meadow pool, collecting marsh gas in a jar. A puzzled and contemptuous cow watches him, deploring, maybe, that he cannot employ his time more practically. Yet by studying the flickering uncanny apparition it was brought inside the framework of chemistry. This was not done by imploring people to give up the idea that the will-of-the-wisp existed.

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[1] Cf. "Privileges and Limitations of Visual Imagery," this *Journal*, 1925, xv.

Similarly, though the visual image may be a slippery customer to deal with, we cannot leave it out of any proposed explanation of skill. Whether anybody would even propose to omit the auditory image from a consideration of skill in musical composition, I do not know. It may be that the relationship between the image and the executive behaviour which accompanies it is the counterpart of the relationship between that experienced fact called a percept and the behaviour which accompanies it.

Perhaps therefore we may call the acts of collection, discrimination, rejection, ordering; in short all those events described on p. 149, as the prelude to skill. They anticipate and adumbrate, as to the experienced hearer, the Prelude to *Tristan* touches upon the whole story. It may be that in the prelude to skill the essential can be usefully distinguished from the inessential preliminaries. When the skilled action is swift, the anticipations are interwoven with the total performance, and never separately articulated, as this spacing-out of the phrases describing them would suggest, though many skilled actions do definitely follow such a prelude.

I will merely mention certain important skills towards the partial analysis of which these distinctions may be of use.

What is mathematical skill?

I am of all men the most unfitted even to speculate concerning the psychological nature of mathematical skill. It demands however the capacity to see relations. This, when exercised in any particular direction for the first time, is called intelligence. It may be assumed that the frequent grasping of relations of a particular type may become a habit. Thus some mathematical skill might conceivably become kinaesthetic knowledge, low grades of which give mere facility in arranging symbols; as, for instance, factorizing an algebraic expression.

Skill in literary composition.

About skill in literary composition I will say little, but (unnecessarily) remind the reader that some books offer to teach the art of writing. Whether many of them do more than to lay down canons, as a book professing to teach tennis might vouchsafe merely a reprint of the rules, I do not know. Mr Beresford, in *Writing Aloud*, and other writers, have occasionally allowed us to see them at the work of composition. And it will please some and offend others if I suggest the complexity of the tasks which would confront a physiologist and psychologist if they tried to write down the problems offered by the fashionable or obsolescent skill of certain 'biographers': Lion Feuchtwanger, Emil Ludwig, André Maurois, Lytton Strachey and, in her latest phase, Virginia Woolf[1]. Everyone will regard them as highly skilful; a few as all-too-skilful.

ARE WE BECOMING MORE CONSCIOUS?

In an essay with this title[2], Mr I. A. Richards suggests that people, particularly some Americans, are becoming more clearly aware of aspects of themselves which until recently have formed an unanalysed background. Some Englishmen think some Americans shallowly enthusiastic about trifles, apt excitedly to emit platitudes about axioms, to blurt out things which 'decent' people would keep to themselves. Such Englishmen probably regard such American minds as having little background, their own extending almost to infinity. On the other hand, some Americans often think that the Englishman is silent because he has nothing to say. As to this English background, the American is not necessarily enthusiastic about it. He may deem it to be of the nature of undergrowth, and rather dank.

Mr Richards and some other writers suggest that we are becoming aware of more of our mental processes, as well as of more things in the outside world. This does not imply merely the uncovering of our minds, as bodies are uncovered, but becoming more clearly aware of certain aspects of our minds, as people have recently become more conscious of their bodies. By parity of reasoning, one may expect an increase in personal mental hygiene, in personal mental cosmetics, and the payment of undue attention to certain aspects of the mind.

The above describes generally a change in usage exemplified by the increasing tendency to describe, analyse and share techniques and skill. The words cunning and craft, long associated with skill, nowadays suggest to many only their secondary, less exalted meanings. It is often felt that a person who keeps skill 'up his sleeve' shows craft in both senses of the word. More significant is the modern attempt to analyse, and thus to destroy as a craft, all craft-skills. That this movement is resented and resisted by the cunning and crafty workers is easy to understand.

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[1] Cf. *Orlando*, London, 1928.

[2] *Psyche*, July 1927, VIII, 26–34.

The impression seems justified that the number of books on 'How to do' this, that and the other, has greatly increased of late. Nowadays it is distinctly fashionable to write about technique. This fashion is worth discussion.

That some of these books are excellent and others pitiful is obvious. Yet the ignominious failure of one person to describe a technique does not prove that the feat is impossible. Many techniques can be adequately described[1]. It is possible to believe that of no valuable technique can the whole story be told. Even here, however, one should make sure that the wish is not father to the thought.

SKILL, TECHNIQUE AND DECENCY.

There is a tendency at present to publish techniques which used to be 'decently' reserved or hidden. Even the discussion of intimate personal relationships is more open. Two recent books, *The Technique of the Love Affair* and the *Art of Making a Perfect Husband,* may be meant only half-seriously. This fact matters little except for readers who respect only dismal writing. Many journals, including a dignified English weekly, answer questions concerning the technique of social relations, and many books here and on the Continent deal with questions of courtship and marriage which until lately were seldom mentioned frankly.

This general interest in technique and skill has suggested the analysability of certain performances which used to be considered unique and incommunicable. Modern language teaching enlists experimental phonetics, with the help of the gramophone and the microphone. Even our sacrosanct English dialects, sedulously encouraged and warmly defended by persons who have spent small fortunes in eliminating their own, are being undermined. Analysis of speech is facilitated, so that it can be consciously altered[2].

A most interesting example of skill-analysis is Dr M. Metfessel's *Phonophotography in Folk Music; American Negro Songs in New Notation*[3]. He writes of "the curious turns and twists and quavers and the intentional striking of certain notes just a shade off the key, with which the negro loves to embellish his songs...an indescribable...subtle effect which only the true 'jubilee voice' can produce...it consists in certain turns, twists...not represented by any musical term." He claims that his phonophotographic technique disposes of the assumption that music and speech cannot be 'notated.'

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[1] Examples cited in *Fitness for Work,* London, 1928, Chapter II; and *Skill in Work and Play,* London.

[2] Cf. Mr A. Lloyd James's broadcast course for schools, *Speech and Language,* British Broadcasting Corporation pamphlets, 1928.

[3] With introduction by C. E. Seashore, 1928, University of North Carolina Press, and Oxford University Press.

That this restless tendency to analyse all skills, and, consequently to experiment with new ones, may lead to some failures, is to be expected. But they would not destroy the truth of the above assertions.

To many, the uncovering of a private technique savours of the indecent. At its mildest, it is like wantonly driving an expensively furnished car with the bonnet lifted. We expect art to be concealed. Grock, when he fails to play his little fiddle while throwing up his bow, retires behind a screen to practise. This we feel to be only proper. And how whole-heartedly we laugh at a conjurer, who, letting us into the secret of a trick, gets it wrong!

Resentment seems most intense where the personal touch is analysed. We smile at the discovery of any deliberate attempt (when it fails) to alter personality. Some years ago, a touring team of debaters met to discuss possible improvement in their methods. Inspired by one university, whose undergraduates had debated casually, nonchalantly and wittily, the team resolved to learn up some more jokes. Why does this amuse us?

Turning to another aspect of the question, it seems probable that since respectable people show to the world more of their bodily and mental selves than was customary, ideas of decency are changing. It has become permissible or even *chic* to run one's personality with little more than its engine and chassis, and the minimum of upholstery.

Tolerance of the public alteration by women of their facial appearance; objection by some persons to certain aspects of this toilet, and by others to other aspects, these facts show the way the wind is blowing. How far this greater openness is a revolt against Victorian customs and to what extent it is a conscious means of attracting attention is an interesting question.

Another fashionable form of this deliberate baring of personality is quasi-cynical reference to one's motives, when announcing decisions. Often, however, this is a subtle defence, for by lightly attacking one's own weak points, an enemy's subsequent thrusts at the same places are deprived of their sting.

While studying the expression of personality by means of the voice[1], I was led to suspect that some would think it indecent to analyse the type of voice acquired by the English ruling classes. Probably it can be analysed. It should not prove more recalcitrant than a negro dialect. But many would feel discomfort at the prospect.

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[1] "Voice and Personality," *Radio Times*, 14 Jan. 1927 (full report unpublished).

THE STYLIZATION OF SUBTLE SKILLS.

It is interesting to note the extent to which skills, becoming 'stylized,' diverge from their original aims. One can gauge the angle of deviation by noting the contempt shown by the practitioner of the skill, in what he holds to be its 'natural' form, for the new upstart 'artificial' style. The speed-skater's attitude towards the figure skater, the *Langlauf* ski-runner's coldness towards 'downhill only' acrobatics; the older French and Italian ballet-dancers' view of the Russians' presentation of *L'Après-Midi d'un Faune* or *The Cat*, are simple examples. This 'what-are-we-coming-to' attitude is not unknown in critics of literature, art and music.

THE FATE OF SKILLS THAT ARE LEFT ALONE.

The present tendency is to examine, analyse, discuss, ventilate and —to borrow an expression from America—'de-bunk' even the most sacred skills. Those not subjected to such treatments may become stylized, causing pride in some and annoyance in others, or stagnant. Stagnation may be deliberately continued, becoming a special style. Ceremonial skills in the church, the army, and the universities exemplify this.

Often stagnation continues to exist in spite of continual demands to end it. Mr William Clissold believes this to be true of the teaching in the provincial universities[1]. (This phrase, the context suggests, describes all English universities, outside London.)

The present article is unsuited for the opening of a hornet's nest. But it seems certain that where there is no effective criticism, as distinguished from the speech habits called 'grousing,' skills will stagnate, even putrefy.

And university teaching notoriously becomes stylized. An unpleasant modern habit is contemptuously throwing to a class shreds of ill-prepared information, with the suggestion, often true, that the 'teacher' is wasting time which would have been more profitably spent in research.

A skill, which, left alone too long, is said to have degenerated, is preaching. There are probably many reasons why it has been left alone, but from time to time it is examined. One of its aspects has lately been discussed at length, by preachers, in a daily newspaper[2].

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[1] *The World of William Clissold*, Book 5, pp. 733 *et seq.*

[2] "The Preacher's Voice," letters in *Manchester Guardian* of 8th Jan. 1929, and subsequent days.

It is doubly significant that the British Broadcasting Corporation is continually asking, and usually in vain, that the newspapers shall review its productions.

PREVALENT ATTITUDES TOWARDS IMPORTANT SKILLS.

Towards important skills, society adopts a graded order of attitudes. Two of these are sharply contrasted. The first encourages persons to harden or stereotype a technique, and to encrust or encapsule it in a mass of ancillary rites, ceremonies, traditions, customs and habits. These secondary techniques in their turn often demand special exponents, to whom the core inside these wrappings and trappings may be inaccessible. Minor priests, footmen, college porters, and some sergeant-majors are examples. They usually develop for their profession sentiments of immense power. These would lead to conservatism even in progressive minds.

The army, navy, and universities are particularly given to skill-mummification. Their resistance to its public unwrapping is easily understood. Mr Robert Graves in *Lars Porsena*, that review—or revue—of a linguistic skill, suggests that though it may be difficult to explain to the next generation what blasphemy meant in our time, criticism of the navy will still be regarded as an obvious example.

The contrasted tendency in our own country, but more especially in less conservative ones, and in those where revolution has followed conservatism, is to take down and rebuild skills of all kinds. That this attack is resented by hieratically-minded practitioners is interesting but comprehensible. Yet not all their pentagrams strewn in his path will deter the modern analyst of movement.

These opponent attitudes can be illuminated in one example. In England there is now a tragic twilight of the old-style music-hall stars. This is attributed to their adherence to outworn techniques and to their younger rivals' incessant experimenting with new ones.

THE SOCIAL MEANING OF THESE CONSIDERATIONS.

This article, beginning as a discussion of skill, ends like an introduction to a sociological discussion. For not less important than the microscopic examination of skill is the macroscopic observation of its place in the life of the people. While in certain respects the first examination seems to be progressing healthily, the second task is less often undertaken.

In conclusion, some social considerations will be indicated.

(1) It is wise occasionally to overhaul our proudly practised techniques. If we do not, others with a different mental outlook will do so. To become sentimentally attached to any skill, except for the sake of ceremonial—an excellent thing in its place—is unwise. Possibly, too, we should realize the danger of becoming victims to an 'obsession to repeat.'

(2) While recording or deploring the subsidence of certain skills it is only right to insist that (*a*) entirely new important skills have recently appeared; *e.g.* many connected with practising, and supplying the mechanical means for, aviation; (*b*) some traditional skills are re-vivified, *e.g.* swimming has been scientifically improved.

(3) Increasingly successful attempts are being made, by example, by precept and by visual demonstration, to share the subtler skills with others. The increasing intellectualization of skill, through rapidly improving technique in photography, cinematography, and in using descriptive words for writing, speech, and broadcasting will popularize widely many techniques. How far such analysis will dissipate the 'personal touch' is at present disputed. Here, the possibility of an unconscious motive in the skill-exponents who discourage analysis cannot be discounted.

(4) The protest, or snarl, at the 'invasion of personality' which even the hint of skill-analysis evokes, is quite healthy. By some innovators in this region it has been ignored or misunderstood. Yet the grounds of the protest are not always clearly stated. Liberty to express one's personality through a skill should be distinguished from licence to muddle through (sometimes miscalled initiative) or to develop consistent but clumsy and wasteful methods of dealing with expensive material.

(5) Attacks upon established skills are useful when they are not mere fulminations but demonstrate the limitations of the skill. For example, criticism of the pianist's skill, with his notes of fixed pitch, by lovers of stringed instruments may thus be profitable.

(6) The separation of visual and auditory knowledge from kinaesthetic knowledge will certainly increase. Understanding the significance of this development is indispensable for the industrial psychologist.

(*Manuscript received* 15 *April*, 1929.)

THE RELATIONSHIP OF SUBJECTIVE EXPERIENCE TO THE PERFORMANCE OF EYE MOVEMENTS

BY M. D. VERNON.

(*From the Cambridge Psychological Laboratory.*)

1. *Introduction* (pp. 161).
2. *Description of experiments* (pp. 161–163).
3. *Kinaesthetic sensations and accuracy of movement* (pp. 163–164).
4. *Kinaesthetic sensations and fixation pauses* (pp. 164–165).
5. *Awareness of meaning and interest* (pp. 165–167).
6. *Awareness of perceptual processes* (pp. 167–168).
7. *Individual differences in performance and introspection* (pp. 168–170).
8. *Conclusion and summary* (pp. 170–172).

1. INTRODUCTION.

IT is well known that in general we are unaware of the exact nature of the movements of our eyes from side to side of the visual field. In reading, the eyes seem to move smoothly along the printed lines, and not in a series of jerks alternating with pauses of greater or less duration. Moreover, since movement is very rapid, it is probable that blurring of the image would occur did not central anaesthesia take place meanwhile. Yet if we glance across the visual field and perceive an object placed at its centre, we are usually unconscious of any fixation pause on that object, and consider that a glimpse was obtained of it in passing. Therefore, in a series of experiments recently performed upon the direction of eye movements and the position of the subsequent fixation points, it seemed of some interest to determine what relationship, if any, existed between the objective performance and the subjective experience which accompanied it.

2. DESCRIPTION OF THE EXPERIMENTS.

The object of the experiments was to discover the accuracy with which the eyes could move from the right to a point on the left, after the successive fixation, from left to right, of a series of stimuli. These stimuli, arranged in pairs of horizontal lines, consisted of two or four block capital letters per line, or five disconnected words, or five words connected together in such a way that the ten words of the pair of lines formed a complete sentence. The letters and words were black, and the field white. The subject was first required to fixate a black cross on a white screen placed in front of the test material; the cross occupied the same position as the first word of the second line. The screen then dropped, exposing the lines of letters or words, and the subject read them, fixating each letter or word separately. By means of a suitable reflecting apparatus, the path of the eye movements could be followed, and the position of the eye at the preliminary fixation compared with its position when fixating the first letter or word of the second line, after the return movement from the end of the first line. Thus the accuracy of return to the beginning of the second line could be determined with some certainty.

With some of this material were presented isolated letters, words, or small drawings (in black) lying between the lines, either directly in the path of the return movement, or just above or below it. Half the drawings were conventional diagrams, and the other half pictures of real objects. Letters and drawings were presented with both the letter series, and words and drawings with the disconnected word and sentence series. In the latter, the pictures were of gradually increasing complexity. It was possible to observe whether the return movements were interrupted by fixation pauses upon these visual obstructions. The subjects were instructed not to fixate anything besides the letters and words of the two horizontal lines.

In order to throw some light on the nature of the tendencies at work in this performance, a fair amount of introspective evidence was collected. It should be noted that all this introspection was not free. A considerable number of questions were asked, such as: "Did you have any difficulty in making the necessary movements? Did you notice the middle letter at all? Did it cause any distraction?" Now these questions are all of the 'subjective direction' type, described by Muscio[1] in an investigation into the influence upon the answer of the form of a question. The accentuation was on the subjective experience; and the answer was more frequently in the affirmative than in the negative. Muscio showed that the reliability of answers to questions of this type was very high. Also the answers to the questions in this investigation varied considerably from time to time, *i.e.* no persistent suggestive influence appeared to be at work. Nor did the experimenter have any preconceived theory as to the results obtained, which would be likely to influence the questions and increase their power of suggestion in any particular direction. Moreover, a good deal of additional elaboration was volunteered which usually rendered it possible to obtain a fair idea of the forces and tendencies at work in the mind of the subject; and it is from this that the following conclusions have been drawn.

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[1] Muscio, B., this *Journal*, 1908, VII.

The experiments were performed by ten subjects, all students of psychology accustomed to introspect, and probably more than usually resistent to suggestion.

3. KINAESTHETIC SENSATIONS AND ACCURACY OF MOVEMENT.

It might be thought that the subject would be completely unaware of the nature of his performance, and that his subjective experience would not correspond in any way with the efficiency and correctness of the movements performed. No estimate of the accuracy of the return movements was either requested or volunteered. But the subjects were asked at some time during each experiment to say if they experienced any difficulty in making the required movements, and to give any reasons for that difficulty if it was felt. Five of the subjects found the letters which were arranged two in a line more easy to read than those arranged four in a line. The letters of the latter series were thought to be so near together that the movements were unduly short (actually each covered more than 3° of arc); it was difficult to pause in the right position, and not to go beyond the correct point. It is probable that this was manifested, at least at first, in hurried, jerky, irregular movements from one letter to the next; no observations were, however, made to determine this. But it is interesting to note that the inaccuracy of the return movements of these subjects was on the average nearly 40 per cent. greater in reading the four-letter lines than in reading the two-letter lines. It is possible that adjustments necessary for the short forward movements and for the long return movements were mutually conflicting and incompatible; hence the judgment that the forward movements were too short, and the inaccuracy of the return movements. Two subjects, however, found the letters arranged two in a line more difficult to read. One of them felt a tendency to drop straight to the second line of letters instead of moving horizontally across. The other found it difficult to move right across the field in one unbroken movement. These tendencies were generally resisted, but they seemed to indicate a difficulty in making long accurate movements, for the return movements were on the average over 40 per cent. less accurate in this series than in the four-letter line series.

In reading the disconnected word series, the return movements of six of the subjects were 40 per cent. *less* accurate than in reading the letter series; but those of four of the subjects were 15 per cent. *more* accurate in reading words than letters. The former subjects reported that the disconnected words were more difficult to read than the letters; the motor habits acquired in the letter series apparently were not utilized, for the movements were felt to be slower and more jerky and irregular. It seems that there was some difficulty in treating the words as isolated stimuli, partly because they were less legible and covered a larger spatial area, and partly because there was an unsuccessful attempt to combine them into the familiar sentence pattern. In the sentence series, the subjects experienced a sensation of ease, as though they were carried on smoothly from one word to the next by means of the connections between the words. In consequence, the accuracy of the return movements of these subjects was 70 per cent. greater in this series than in the disconnected word series. It seems that the habitual easy to and fro movements which have been developed in normal reading were utilized in this series, while they could not be employed in the disconnected word series.

The other four subjects, however, were much less variable, both in objective performance and in subjective experience. In general they reacted to the words in much the same way as to the letters—that is to say, they treated them as isolated stimuli to be fixated separately. The average inaccuracy of their return movements was 15 per cent. *less* in the disconnected word series than in the letter series; but this difference was probably not significant. In the sentence series, deeply-rooted normal reading habits seem to have been inoperative, for the inaccuracy in this series averaged 25 per cent. *more* than in the word series. The tendency appears to have been to separate the words in this series also; and the effort to fixate all the words, according to the instructions given, broke down the normal reading habits.

4. KINAESTHETIC SENSATIONS AND FIXATION PAUSES.

If we now study the interruption of the return movements by the fixations of the visual obstructions lying between the lines of the text, we discover at first little parallelism between the actual number of pauses in the return movements and the objective performance of the subjects. The latter constantly over-estimated their ability to inhibit fixation of the visual obstructions during the return movement. They were in general aware of fixation pauses of considerable duration, when the obstruction was studied in some detail. These pauses may have been involuntary in their inception, but they were to some extent continued voluntarily. Involuntary pauses of very short duration were not usually cognized; the subject considered either that he had perceived the object peripherally while fixating the letters or words of the text, or else that he had glanced at it in passing without stopping to fixate it. A real difference appears to have existed between fixation pauses described as 'looking at' something, and 'glancing at' it during movement. It is not possible from these observations to assign definite time limits to either type, but it seems clear that both the kinaesthetic sensations and the visual perceptions experienced were qualitatively different in the two cases. The eyes were judged to be moving throughout the short pauses, but to remain at rest during the long pauses. In the latter the visual obstructions were examined in detail, their full meaning and significance determined, and a position assigned to them in the complex association patterns of previous experience. But during the unconscious fixation pauses, perception was vague, so much so that it was often thought to be peripheral rather than foveal. Details were omitted or altered, and sometimes a wrong meaning or name was assigned. It is probable that these short fixation pauses were of the type normally employed in that vague perception of our surroundings which only enters the fringe of consciousness, to form the background or setting for the objects which we scrutinize and attend to in detail.

The forward movements made in reading the top line of letters or words were also much affected by the visual obstructions. It was rather difficult to distinguish clearly the abnormal hesitations and regressions from normal fixation pauses on the letters or words, unless the former were prolonged. However, it appeared that on the whole it was only the very long hesitations and regressions of which the subjects were aware. But regressions caused by inability to read the text, whether as the result of illegibility of the words, or of confusion of the meaning by the obstructions, were usually cognized; and so also were some of the hesitations which occurred early in the experiments as a result of unfamiliarity with the required movements. Otherwise, the subjects generally considered that they did not fixate the obstructions during the forward movements, unless the pauses were very prolonged, but perceived them peripherally.

5. AWARENESS OF MEANING AND INTEREST.

If the introspective data so far described appear to bear little relationship to the actual processes of movement and fixation, it must be remembered that, after the early stages of the experiments, kinaesthetic sensations were rarely at the focus of consciousness. But an important class of tendencies, connected with the meaning and interest of the text and obstructions, were much more completely cognized. The subjects were in general fully aware of their perceptions of the material presented, of the interest aroused, and of the connections and conflicts between the organized arrangements of meaning which constitute the patterns of language and thought. The relation between these tendencies and the objective performance was not always clear at first; but upon further consideration, the effects of these tendencies upon motor behaviour proved to be of great importance, particularly with certain subjects. The following principal facts emerged:

(1) More than twice as many pauses were made on word obstructions in the disconnected word series than on word obstructions in the sentence series. Yet these obstructions were thought to interfere far more with the reading of the sentences than of the disconnected words.

(2) The drawings of real objects were considered to be intrinsically interesting and attractive, but not to interrupt and upset the reading of the sentences so frequently as had the word obstructions. In this series there were slightly fewer pauses in the return movements on the pictures than on the words. But the pictures were reported to cause some distraction in the letter and word series, where they were responsible for 11 per cent. more pauses in the return movements, on the average, than were the word and letter obstructions (in the central position, directly in the path of the return movement).

It was pointed out above that the sensation of ease and smoothness of movement reported by most subjects denoted the existence of strong motor habits in reading connected sentence material, as shown by the accurate adjustment of the eye movements in the sentence series. It seems reasonable to suppose that these motor habits were also responsible in part for the comparative freedom from interruption of the return movements in this series. But these were not the only habits of importance. The sentence forms the accustomed unit of speech, and, probably, also of thought, at least of verbal thought; and to many individuals the isolated word is not a separate unit, but part of a potential phrase or sentence. Hence the tendency constantly at work to connect isolated words into some group or organization, and the feeling of interest or satisfaction when this was possible; hence also the conflict experienced when the word obstructions could not be fitted into the sentence pattern with which they were presented. In the last case, well-formed motor habits continued to guide and direct the eye movements; but the organization and arrangement of the meanings of the words was prevented, with the consequent feeling of distraction. In the disconnected word series, the obstructing words and the words of the text were all of equal value, either as isolated units or as parts of a potential sentence whole. There was a strong tendency to fixate the obstructing words in order to find out their meaning, but there was no struggle between opposing meanings. In this series the chief difficulty, at least for some of the subjects, was a motor one, resulting from the mutual inhibition of the motor habits appropriate to the successive fixation of isolated stimuli, and the deeply-rooted habits of normal reading. This increased inaccuracy more than it increased interruption of the return movements.

While the subjects regarded the pictures of real objects as isolated units, they found that the latter did not conflict with the reading of the sentences, although they were intrinsically more interesting. That is to say, the pictures did not interfere with the connected sentence pattern because they belonged to a different mental category, and the subjects felt no tendency to bring them into the sentence pattern. But if they either attempted to establish a connection or relationship between the pictures and the sentences with which they appeared, or allowed their attention to centre upon the relationships of the pictures to each other, a struggle was experienced, with the frequent result that all meaning and comprehension of the sentence content was lost. The accuracy and continuity of the eye movements were still remarkable; clearly the necessary movements could be executed effectively under very considerable distraction. For the disconnected word series presented a different case. Here there was no conflict. The intrinsic interest of the pictures was alone sufficient to fill the attention and impair greatly the accuracy and regularity of the eye movements. It is interesting to note, however, that the number of pauses on the pictures in the return movements was greater in the disconnected word series, where there was a connection between the successive pictures, than in the letter series, where there was none; and this result agrees with the comparative interest in the two sets of pictures which was reported by the subjects.

6. AWARENESS OF PERCEPTUAL PROCESSES.

The conventional diagrams aroused little interest and did not cause nearly as much distraction as the pictures of real objects. They were thought to interfere considerably with the reading of the letters and to some extent of the disconnected words; but not much with the sentences. Actually the number of pauses on diagrams was large in both disconnected word and letter series, sometimes greater and sometimes less than the number of pauses on the pictures of real objects. The number was small in the sentence series, but considerably greater than the number of pauses on pictures of real objects.

If the reasons given for the effect of the diagrams be now considered, three factors of importance appear, all of which were related to the perceptual processes. The first was the visibility of the obstruction, its distinctness from its surroundings. The diagrams were rather larger and blacker than many of the other visual obstructions; moreover, they stood out from the words and letters of the text by reason of their contrasted form. They exerted a kind of involuntary attraction on the eyes, as appears from the reports of 'something there,' 'something to be skipped.' The second factor was the structural simplicity or complexity of the object. The diagrams were almost all structurally simple, as compared with the pictures of real objects. But more important than this was the third factor, the ease or difficulty with which they could be apprehended. None but the simplest diagrams, a cross and a square, were familiar; they could not be named or fitted into some previously formed concept or organization of mental material. These two factors were in opposition. Some subjects reported that fixation of the diagram obstructions could not be avoided, because the latter were ambiguous, and could not be apprehended without long scrutiny. Other subjects could perceive them clearly, even in peripheral vision, on account of their structural simplicity; and thus felt little tendency to pause on them. But since they possessed no meaning and little interest, they generally disturbed only the motor processes, and rarely the thought or language processes.

The picture obstructions generally possessed good visibility, with the exception of those in the sentence series, and were readily apprehended; but they were structurally complex. The visibility and the structural complexity both strengthened the tendency to fixate these obstructions; for it was impossible not to see that they were there, but not always easy to see what they were.

7. Individual differences in performance and introspection.

It is interesting to compare the various objective results and introspective record from the point of view of individual differences. Four of the subjects showed considerable inaccuracy throughout, and made a large number of pauses in the return movements. The inaccuracy and the number of pauses were both greatest in the disconnected word series and least in the sentence series. These subjects experienced considerable difficulty in reading disconnected words; they found it harder to form regular motor habits in reading this type of material than in reading the letters, since the habits appropriate to the successive fixation of isolated stimuli were destroyed, and normal reading habits could not be utilized. Moreover, the tendency to fixate the word and picture obstructions was very strong. In reading the sentences, a kinaesthetic sensation of ease and smoothness was experienced; but a feeling of conflict was aroused by the inability to fit the obstructing words into the meaning pattern. The pictures caused comparatively little distraction until it was realized that they too possessed an organized relationship; interest in this relationship then completely superseded apprehension of the sentence content. It was concluded that these individuals on the whole adopted a subjective attitude towards the experiments. They were concerned with meaning, significance and interest rather than with objective accuracy of movement; consequently they had some difficulty in forming motor habits appropriate to the fixation of isolated stimuli, but placed great reliance upon the well-formed habits of normal reading.

Three other subjects presented an almost complete contrast. They were accurate throughout, and made few pauses in the return movements. Moreover, the inaccuracy and the pauses occurred with comparative regularity throughout, though the former was slightly greater in the sentence than in the other series. It appeared that these subjects reacted much more objectively to the experimental conditions; that is, they concentrated upon the effort to perform the required movements regularly and accurately. Consequently they were able to develop habits of movement from one point to the next which were well adjusted both for the letters and for the disconnected words. These, rather than normal reading habits, were utilized in the sentence series; for the subjects reported that, with some difficulty, they read the words as isolated units, and not as parts of the sentence whole. Moreover, these motor habits precluded the interruption of the return movements, even by obstructions of considerable interest. The peculiar conflicts between meaning organizations were infrequent in occurrence, presumably because more attention was paid to the motor processes, and less to meaning and significance.

The remaining subjects presented somewhat anomalous cases. Two of them adopted the objective attitude as a rule, and paid much attention to regularity and accuracy of movement. But they possessed poor motor control, and though they made few pauses in the return movements, the inaccuracy of the latter was considerable. But apparently motor control was fairly adequate when the deeply-rooted habits of normal reading were operative, as in the sentence series, and to some extent with one subject in the disconnected word series.

The last subject adopted a very markedly subjective attitude to most of the experimental material. He was particularly interested in constructing connections and relationships between the words and pictures. But his accuracy of movement was fairly high, and he made very few pauses in the return movements. Accuracy appeared to be approximately the same in the letter, word and sentence series, presumably because these interested him equally (he reported considerable interest in both the letters and disconnected words). But it differed considerably for the different visual obstructions, because the intrinsic interest of these varied greatly, from the diagrams, which he considered completely uninteresting, to the connected pictures, which he related most ingeniously to the words and sentences with which they appeared. His performance also varied from day to day, according to various moods and affective states to which he was liable. But it appeared from his introspections that he also paid much attention to his objective performance, and made considerable efforts, in particular, to avoid fixating the obstructions during the return movements. Moreover, he possessed excellent motor control of the eyes. It seems that the introspective evidence which indicated a subjective attitude towards the experimental material did not altogether conflict with the objective results. But this individual probably exerted an unusually large amount of conative effort; hence he was able to carry on simultaneously the pursuit of accuracy and of meaning, while the other subjects, in whom conation was less powerful, found these pursuits mutually exclusive.

8. CONCLUSION AND SUMMARY.

In conclusion, it appears that the subjects were unaware of the exact nature of their eye movements. They could only report the fixation pauses of greater duration, and the more considerable derangements of the movements, *i.e.* by long regressions or hesitations. These reports were not reliable, however; and rapid regressions and short hesitations went unnoticed. But, on the other hand, kinaesthetic sensations of the general ease and regularity of movement were closely related to objective performance, particularly in comparing the various results of the same subject. This was especially noticeable in the correspondence of ease and accuracy of movement in the letter, disconnected word and sentence series. It is not easy to account for the fact that certain subjects found the longer movements of the two-letter line series more difficult, and others the shorter movements of the four-letter line series; but reasons have been advanced for the inaccuracy of movement in the disconnected word series, and the accuracy in the sentence series. Comparatively little attention was paid to these kinaesthetic sensations. The individuals characterized by a subjective attitude were concerned with the content of the experimental material; while those who adopted the objective attitude thought more of *what* they did than of *how* they did it.

Hence much fuller and probably more reliable data were obtained as to the effects of meaning and interest; and as to the degree to which the mind was occupied by the respective meanings and interests of the text and obstructions, and the rivalries between the two. It was pointed out that much light was thrown upon the various anomalies in the objective behaviour, particularly with regard to the number of fixation pauses in the forward and return movements, by a consideration of these competing tendencies, and also of three factors appertaining neither to cognition nor to kinaesthesis, but to the actual perceptual processes. Unless all these factors are taken into consideration, it would be impossible to account for the relative effects of letter, word, diagram and picture obstructions. It was shown, also, how the relative results of the different subjects were explained to a great extent by a study of the tendencies and attitudes appearing from introspection. No complete conception of their motor behaviour could be arrived at without taking into account other unconscious tendencies, such as general motor control of the eyes and liability to impairment of accuracy with fatigue. We are, however, justified in advancing the data obtained from subjective experience as bearing a close relationship to the objective performance, and as elucidating to no small extent the reasons for the varieties in that performance.

To sum up:

A number of experiments were carried out upon the accuracy of the performance of a series of eye movements from right to left of the visual field, and the frequency of their interruption by fixation pauses on a variety of visual obstructions placed in their path. Introspective data were obtained, and their relationship to the objective performance considered.

Kinaesthetic sensations of ease or difficulty in making the required movements were closely connected with accuracy and inaccuracy of performance. Accuracy was greatest when the deeply-rooted motor habits of normal reading were operative; and was least when these were inhibited by habits acquired temporarily in the successive fixation of isolated stimuli.

The subjects were usually unaware of the shorter fixation pauses during which perception and apprehension were partial and vague; but they reported the longer pauses which were accompanied by close scrutiny and complete apperception of the visual object.

Introspective evidence revealed the importance, particularly to certain subjects, of the meaning and interest of the text and visual obstructions, and especially of the conflicts and mutual inhibitions of these meanings and interests. Perceptual factors connected with the visibility, structural simplicity and ease of apprehension of the visual obstructions also affected the objective performance.

Certain of the subjects, who were characterized by a subjective attitude towards the experiments, were more influenced by the above tendencies than others who adopted a comparatively objective attitude; and the performance of the former was more inaccurate and variable than that of the latter.

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EXPERIMENTS ON THE INFLUENCE OF THE AMOUNT OF REWARD ON LEARNING IN YOUNG CHICKENS

By G. C. GRINDLEY.

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1. *Introductory* (p. 173).
2. *Exp.* 1*: Food-tray in sight* (pp. 173–175).
3. *Exp.* 2*: Food-tray out of sight* (pp. 176–177).
4. *Exp.* 3*: Learning with no reward* (pp. 178–180).
5. *Conclusions* (p. 180).

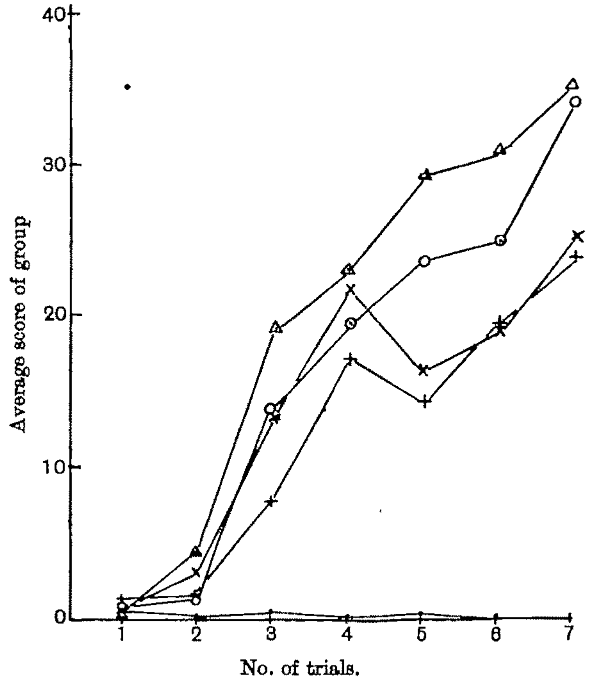
1. INTRODUCTORY.

In many experiments on learning in animals the food-seeking tendency is used as an incentive, and it has been shown (1) that in these cases the rate at which an animal learns is dependent on its state of hunger. It might be expected that the rate of learning would also be dependent on the amount of food given as a reward for successful performance, but this problem, although it is of considerable theoretical importance, does not seem to have been investigated. Yerkes and Dodson (2) have, however, described some very interesting experiments on the influence of the amount of punishment given for unsuccessful performance on the rate of learning in mice, and Cole (3) has studied the same problem in chickens; while Hoge and Stocking (4) and Dodson (1) have compared the effectiveness of certain rewards with that of certain punishments, and Stone and Sturman-Humble (5) have compared the value as incentives of food-seeking and sex.

The present paper describes briefly some experiments on the influence of the amount of food given as a reward on the rate at which young chickens learn certain relatively simple performances.

2. EXP. 1: FOOD-TRAY IN SIGHT.

The apparatus in this experiment consisted of a passage 4 ft. long and 8 in. wide at one end of which was a release-box. In the preliminary training the chicks were trained individually to run out of the release-box immediately it was opened and to look for food anywhere in the passage. Seventy chicks were trained in this way from an age of five days to an age of ten days, each chick being given fifteen trials. Each chick was then tested by measuring the times taken by it in four trials to find a grain of rice placed at a fixed spot near the middle of the passage. From the results of this test the fifty 'best' chicks were selected, and they were divided into five groups of ten.



Curves 1.

• Group 0. + Group 1. × Group 2. ⊙ Group 4. ◬ Group 6.

In the actual experiments a small wooden tray was placed at the end of the passage remote from the release-box, and a certain quantity of food was placed on this tray; the amount of food being the same for each chick of a given group, but differing from group to group. A projecting ridge on top of the tray hid the food from the chick until it was within about 4 in. of the tray. The rewards given to the chicks of the various groups were as follows: Group 1, one grain of boiled rice; Group 2, two grains; Group 4, four grains; Group 6, six grains; and Group 0, no reward.

Learning curves were obtained in the usual way, each chick being tested only once in each day. All the chicks were kept together between experiments, and when they had been tested they were given a large meal of rice. At other feeding times they had no rice. In this way it was arranged that the chicks of the various groups should have as nearly as possible the same appetite for rice at the tests. The experiments were always made at a time when the chicks were hungry.

A difficulty which is often encountered in experiments on individual chicks is that of 'loneliness.' To obviate this four chicks (which, of course, were not taken from the fifty used in the experiments) were tethered with string at one side of the passage. It was found that after the first few days of the preliminary training the tethered chicks remained quiescent, and that the chicks taking part in the experiment took little notice of them.

In each test a chick was given a 'score' equal to one hundred times the reciprocal of the time in seconds taken by it to reach the food (or, in the case of the chicks of Group 0, the time taken to reach the tray) after the opening of the release-box. Thus a chick which pecked at the food after five seconds was given a score of 20. If a chick did not find the food in 120 seconds it was gently pushed along to it by the experimenter, and given a score of zero. Some of the chicks were found to develop a dislike for rice during the experiments. Any chick which failed on more than one occasion to eat the whole amount on the tray was disqualified, and all its scores were neglected. In all, thirteen chicks were disqualified: four from Group 1, one from Group 2, four from Group 4, and four from Group 6. The experiment began on the twelfth day after hatching and was continued for seven days.

Results.

The average scores obtained by the chicks of each group on successive days are shown in Curves 1. It will be seen that, under the conditions of this experiment, there is a marked increase in the rate of learning with increase in the amount of the reward. If we take the total of the average scores of any group as a rough measure of the rate of learning of that group, we find that this rate is about 60 per cent. higher for the chicks of Group 6 than for those of Group 1.

Towards the end of the experiments it was noticed that most of the chicks of Groups 4 and 6 walked or ran briskly down the passage, showing some signs of excitement, while the chicks of Groups 1 and 2 usually proceeded in a more leisurely way. But there were wide individual differences in behaviour.

3. Exp. 2: Food-tray out of sight.

It was thought that the increase in the rate of learning with the amount of the reward in the experiments just described might be due, wholly or in part, to the fact that in those experiments the food-tray could be seen immediately the release-box was opened. It seems possible that a chick which has pecked six times in rapid succession at food on a tray will 'associate' the sight of the tray with food more strongly than will a chick which has only pecked once, and that this kind of association may operate even when the tray is seen from a totally different aspect (*e.g.* from the release-box). On the other hand, if there is any influence of the amount of the reward on maze learning, then this influence must be due to quite another kind of 'association'—namely, associations between objects which are seen (or actions which are performed) *before* the food is reached and food.

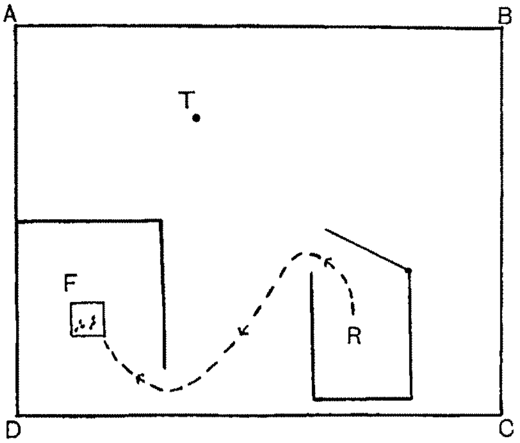


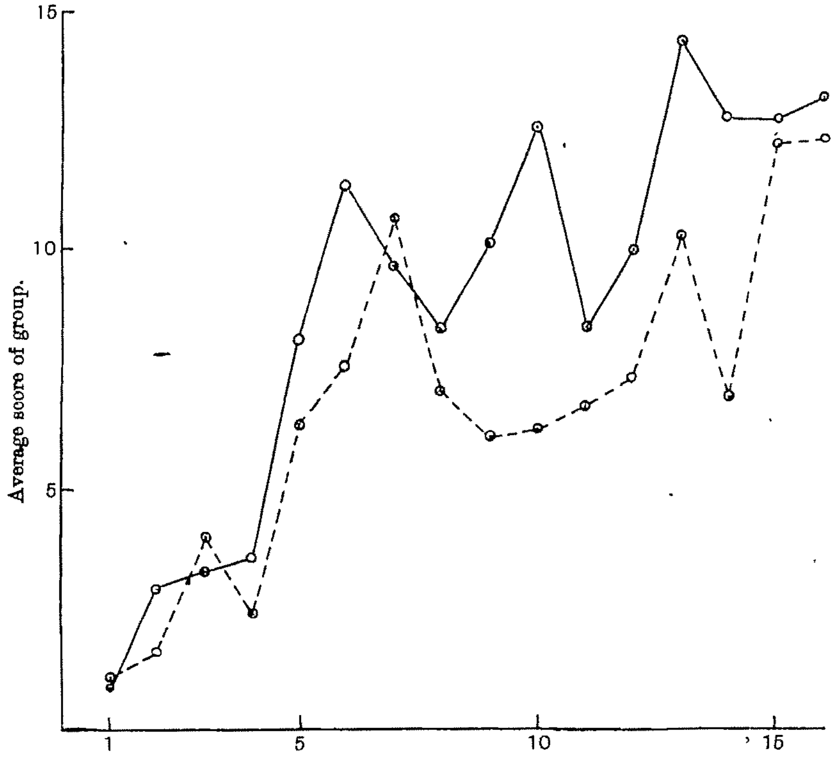
Fig. 1.

An experiment was therefore carried out with a very simple form of maze. A plan of the apparatus is shown in Fig. 1, the enclosure *ABCD* measuring about 5 ft. × 4 ft. *R* is the release-box, *F* is the food-tray, and *T* is the position of two tethered chicks. There was no preliminary training.

Two groups, each of ten chicks, were employed. The chicks of Group 1 received one grain of rice, and those of Group 6, six grains. The experiments began on the twenty-seventh day after hatching and continued until the thirty-second day. Care was taken to ensure that the chicks of the two groups had, as nearly as possible, equal appetites for rice at each test.

The learning curves are shown in Curves 2 A, scores being reckoned as before. The difference between the total scores of Group 6 and Group 1 is about 29 per cent.

The experiment was repeated (with certain minor modifications which need not be described) with another twenty chicks. The results are given in Curves 2 B. The difference between the total scores is in this case about 23 per cent.

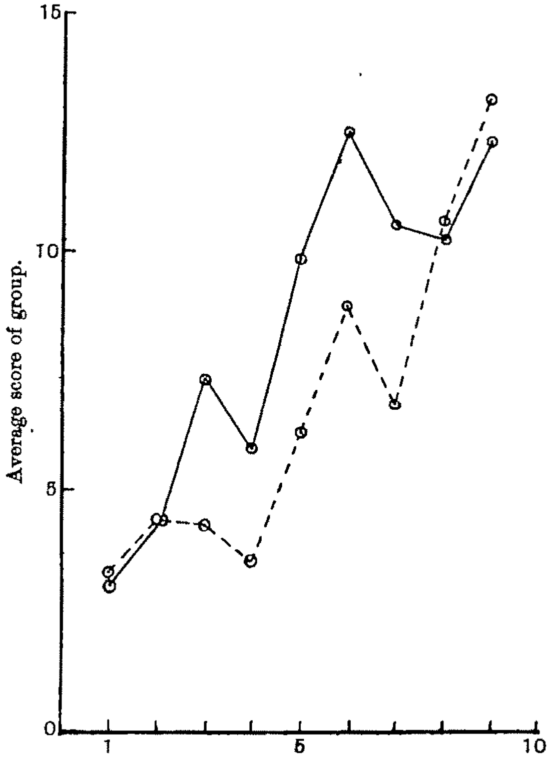


Curves 2 A.
——— Group 6. - - - - Group 1.

All the chicks used in these experiments retained their appetite for rice, and none was disqualified. The chicks receiving a reward of six grains were not noticed, on the whole, to be more excited during the experiments than those receiving only one grain. But there were wide individual differences, and judgment on this question could not be made with any certainty.

4. EXP. 3: LEARNING WITH NO REWARD.

In this experiment the apparatus used was that of Exp. 1, but a flat piece of wood with a few grains of rice on it was substituted for the food-tray, and a sheet of plate-glass was placed across the passage a few inches from this, so that the chicks could see but could not reach the food. The chicks were experimented on in pairs, and tethered chicks were therefore not required.

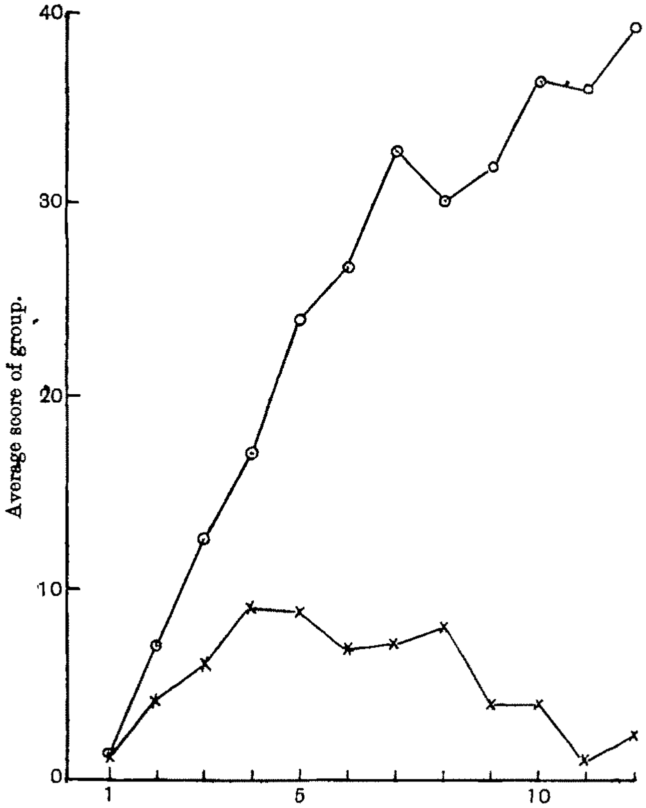


Curves 2 B.

——— Group 6. - - - - Group 1.

Learning curves were obtained in the usual way, the scores being based on the time elapsing between the opening of the release-box and the instant when the slower of the two chicks reached a point a few inches from the glass. Each pair of chicks was given twelve trials in rapid succession. In a control experiment with other pairs of chicks the glass was removed (so that the chicks could reach the food), but the conditions were otherwise identical. The experiments were performed on the twenty-fifth day after hatching.

The results are shown in Curves 3. Curve A shows the average scores of five pairs of chicks receiving no reward, and Curve B shows the average scores of five pairs of chicks allowed to reach the food.



No. of trials.

Curves 3.

× Curve A (No Reward). ⊙ Curve B (Reward).

It will be seen that the chicks receiving no reward improved for several trials and then grew worse. This result might be expected on grounds of common sense, but it is difficult to explain on some of the current theories of learning. The initial rise of the curves shows that the occurrence of the 'consummatory response' is not essential to the early stages of learning. The subsequent fall suggests a process analogous to

Pavlov's 'extinction' of conditioned reflexes (6), and it may be taken as further evidence of the inadequacy of mere repetition in the formation of habits.

5. CONCLUSIONS.

(1) The results of Exps. 1 and 2 support the view that, under the conditions of these experiments, the amount of the reward has some influence on the rate of learning. In Exp. 2, where the conditions were those of simple maze learning, it was found that a sixfold increase in the amount of the reward produced an increase of about 25 per cent. in the rate of learning. But it must be admitted that the individual differences between the chicks were so great that the experiment (although forty chicks took part in it) was not sufficiently accurate to establish this result with certainty.

(2) Exp. 4 shows that, under certain conditions, a transient kind of learning occurs when no reward is given.

I wish to express my thanks to Professor A. P. Chattock and Mr M. H. Carré for suggestions and help.

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(*Manuscript received* 1 *December* 1928.)

THE QUANTITATIVE ESTIMATION OF THE SENSATION OF COLOUR

By R. S. MAXWELL, M.A., B.Sc.

1. *Introduction* (pp. 181–182).
2. *Preparations and experimental work* (pp. 182–184).
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7. *Summary* (p. 189).

1. INTRODUCTION.

L. F. RICHARDSON[1], in his paper on "Quantitative Mental Estimates of Light and Colour," describes some experiments in which he obtained the opinions of a large number of persons as to the degree of redness in a certain pink. In the analysis of the results, the subjects were classified in different groups, *e.g.* Fellows of the Royal Society, students working for the Intermediate B.Sc., etc. It was found that for groups containing eight or more members, the group-mean was approximately constant for the same sex. Therefore it appeared that in any subsequent research it would be sufficient to work with one group provided that it was not too small.

Richardson stated concerning his experiments: "the pink, made by mixing $\frac{2}{3}$ of scarlet with $\frac{1}{3}$ of white on a colour wheel, was judged mentally to be at the following positions on a scale in which 0 is white, 100 is scarlet. Mean estimate by men 43, by women 50. There is thus a remarkable disagreement between angles and mental estimates."

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[1] This *Journal*, 1929, xx (1), 27–37.

This present research was undertaken in order to investigate further the relationship between the angular percentage of red in the colour wheel and the mental estimate of the position of the pink on the scale of redness.

2. PREPARATIONS AND EXPERIMENTAL WORK.

(*a*) *Preparation of the discs.*

A series of colour wheels were prepared by mounting red blotting paper (Ford's blotting—428 mill—red) on some pure white "mounting board," with Kodak photographic mounting paste. The blotting paper was cut into the shapes shown in Fig. 1. The diameter of the white card was 30 cm., that of the larger red circle was 22·5 cm., and that of the smaller red circle was 15 cm. Table I shows the angle and the percentage angle of red in the middle zone of the disc.

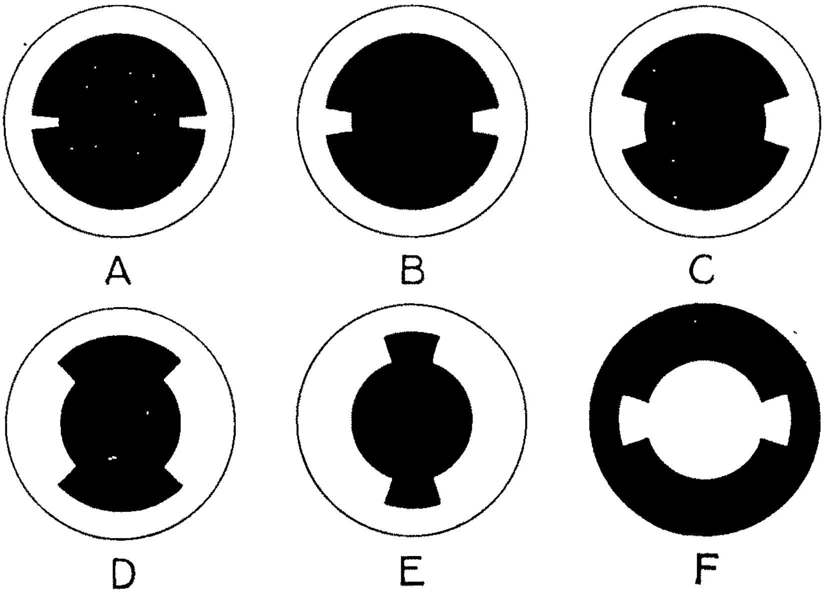

Fig. 1.
Disc H is just like Disc C except that the centre is Blue instead of Red.

The pink coloration of the middle zone is produced when the disc is mounted on an electric motor, and rotated at a sufficiently high speed to obviate any flicker due to the rapidly alternating patches of red and white.

Table I.

Identifying mark of disc	*A*	*B*	*C*	*D*	*E*	*F*	*H*
Angle of red on the disc in degrees. Remainder of 360° is white	342	324	288	180	72	288	288
Percentage angle of red in disc	95	90	80	50	20	80	80

(*b*) *The observers.*

The group of observers who made the estimates of the pink of the discs were boys from the upper forms of Manor House School, Clapham Common, S.W. 4. The ages of the boys ranged from 14 to 18 years, the average age being 15·8 years.

No relationship between age and ability to make reasonable estimations could be observed, the younger boys being quite as able to grasp what was wanted of them, and to do it, as those who were older.

The experiments were conducted on four separate days, and it was not possible for all the boys to be present each time. Thirty-five boys in all made estimations, but the number present on any one day was thirty.

(*c*) *Conduct of the experiments.*

The boys were seated in a class-room which was well lighted with four windows. All the days of the experiments were bright, but no direct sunlight came into the room. The motor on which the discs were rotated stood on a desk facing the boys and it could be seen comfortably by all. The class were only allowed to see the discs while they were in rotation, and when it was necessary to change a disc a cloth was held up which completely screened the disc from the boys.

The distance of the boys from the disc varied between 1 and 7 metres, but on analysis of the results no connection between distance and estimation of pink could be observed.

The method adopted for recording the observers' estimates was the same as that used in Richardson's experiments with the exception that there were no written instructions. Each boy was given a slip of paper on which was drawn a line 10 cm. in length with 'RED' written at one end and 'WHITE' at the other. These slips were reproduced by means of a hectograph. Brief oral instructions were given, care being taken to emphasize that the individual opinions of the boys were required, that each opinion was as good as any other, and that there was no 'right answer.' The boys were told to make a mark on the line, where they felt that the pink came on the scale of redness between the white and the red. No one complained that the task was impossible. The slips were collected after each disc had been shown, so that the boys could obtain no information from their own previous estimates, except by memory.

The actual experiments took place on four different days, and on each day five discs were shown to the class. Since the main object of the research was to find the relationship between the angular percentage of red and the mental estimate of the shade of pink produced, the usual reversals were made to eliminate fatigue and practice. The series of discs *A–E* was shown on three occasions at intervals of four days; the details may be seen by reference to Table II. On the other day observations were made on miscellaneous discs having a central zone of 80 per cent. colour. Further particulars will be found in Table III.

The results of all these experiments are discussed in the next section.

(*d*) *Conversion to numbers.*

The slips were measured by the author and the measurements converted into numbers. Following Richardson, the 'estimate' of the pink is defined to be "the percentage of the length of the line from the end marked white up to the mark." The scale is:

White = 0 Red = 100

and since the total line is 10 cm. in length, it was only necessary to measure the portion from the end marked WHITE to the mark in millimetres. "Throughout the arithmetic, numbers are rounded off to the nearest unit percentage, or if exactly midway, then to the neighbouring integer which is even."

3. ANALYSIS OF RESULTS.

(*a*) *Relationship between percentage angle of red and mental estimate of pink.*

The results of the experiments made on the first, third and fourth days are summarized in Table II.

The following points emerge from a consideration of that table:

(1) The distribution of estimates is fairly regular. In no case does the median differ from the mean value by more than two units.

(2) The standard deviations from the mean value are less for the extreme shades, *i.e.* for the very pale pink and the very deep pink, than for the intermediate shades. This fact was very noticeable when measuring the slips. Most of the marks made by the boys showed very good agreement among themselves for these extreme shades, but there were many rather wild guesses made when a disc such as *C* was shown.

(3) There is a definite dependence upon the order in which the discs are shown.

A possible explanation is as follows: On the first and fourth days disc *A* was shown first, and this was a very deep pink. The feeling was to make a mark at a certain distance from the *red* end of the line. On the third day, disc *E*—a very pale pink—was shown first. In this case, the mark was made at a certain distance from the *white* end.

Table II.

Number of boys who made estimates each day = 30.

Date	Sequence in which discs were shown to class	Estimates	Identifying mark of disc				
			A	*B*	*C*	*D*	*E*
			Percentage angle of red in disc				
			95	90	80	50	20
1928 13 July, 10 a.m.		Greatest	94	83	67	40	14
		Median	86	66	50	23	7
		Mean	**86**	**66**	**48**	**24**	**8**
	→	Least	72	44	12	8	1
		Standard deviation of individual from mean of class	6	9	12	8	4
1928 17 July, 10 a.m.		Greatest	99	96	84	48	41
		Median	94	82	67	30	10
		Mean	**94**	**83**	**67**	**32**	**11**
	←	Least	86	65	52	18	3
		Standard deviation of individual from mean of class	3	8	10	9	8
1928 21 July, 10 a.m.		Greatest	98	91	74	44	24
		Median	90	73	50	20	6
		Mean	**89**	**72**	**48**	**21**	**8**
	→	Least	75	51	31	9	2
		Standard deviation of individual from mean of class	6	10	10	8	5
General Mean	$\frac{1}{2}[\frac{1}{2}(\rightarrow+\rightarrow)+\leftarrow]$		**91**	**76**	**58**	**27**	**10**

The general tendency seemed to be to mark off rather a large difference for the first one and then to 'push on' further for each succeeding disc.

In any case the values obtained when proceeding from pale pink to deep pink were consistently higher than those obtained when the discs were shown in the other sequence.

(4) The results obtained when the first series of experiments was repeated again on the fourth day, show good agreement between the two sets of observations. It thus appears that the effect of practice in making these mental estimates is not so pronounced as the effect of sequence.

(5) For all shades of pink the mental estimate is considerably lower than percentage of red actually contained in the red and white of the colour wheel.

A given amount of white thus appears to have more influence in governing the shade of the resulting pink than does the same amount of red. This result is in agreement with the photometric measurements described in section 5.

The graphical relationship between the percentage angle of red and the mental estimate is shown in Fig. 2.

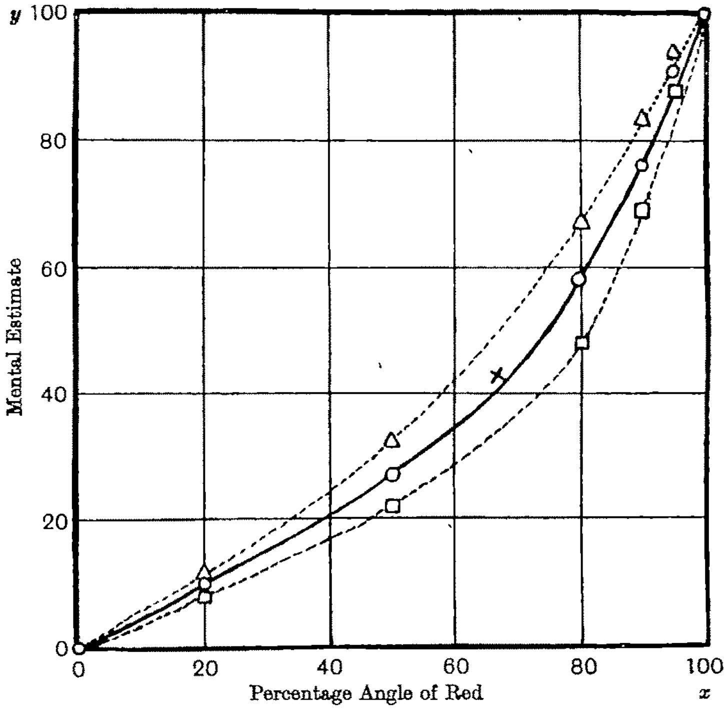

Fig. 2.

□ Mean of values obtained on July 13 and 21.
△ Values obtained on July 17.
○ General Mean values.
× Richardson's value for men.

It has been found that if

Percentage angle of red $= x$,
and Mental estimate $= y$,

then the mean values of the experimental points lie on the hyperbola

$$(x - 156)(y + 56) = -8736.$$

It will be noticed that the mean value obtained by Richardson for men using a slightly different colour red paper also lies very close to this mean curve.

(*b*) *Miscellaneous discs of* 80 *per cent. colour.*

The experiments on the second day were made in order to test the effect of (α) an interchange in the positions of the red and the white in disc *C*, and (β) the use of a different colour—in this case blue.

Table III shows the tabulated results of these experiments.

It will be noticed that:

(1) The standard deviations for all the experiments made on this day were fairly large, many erratic estimates being made.

(2) The two estimates for disc *F*, namely 45 and 53 (mean 49) are lower than those for disc *C* namely 55 and 51 (mean 53).

Probably in order to obtain a true mean value for the estimates, a second set of discs ought to be prepared of the type of *F*, and two further sets of readings taken, one for each sequence of discs.

(3) The estimate for the blue disc *H* (60) is higher than any other estimate made on this day, but it lies between the values for the red disc *C* found on the third day (67) and the first and fourth days (48).

Table III.

Number of boys who made estimates = 30.

			Identifying mark of disc				
			F	*C*	*H*	*C*	*F*
			Description of disc				
Date	Sequence in which discs were shown to class	Estimates	Red rim white centre	White rim red centre	White rim blue centre	White rim red centre	Red rim white centre
1928 14 July, 9 a.m.		Greatest	81	80	78	83	60
		Median	52	52	58	51	44
		Mean	**53**	**55**	**60**	**51**	**45**
	→	Least	30	33	38	32	23
		Standard deviation of individual from mean	10	11	10	11	9

In order to study the effect of colour in any detail, a large number of further experiments would have to be made. Probably the most fundamental results would be obtained on working with black and white, and various colours could then be tried afterwards. The reason why the present writer used red was because Richardson had done so, and it was deemed desirable to extend his experiments. The reason that Richardson chose red was to provide an answer to the statement of William James[1]. "To introspection, our feeling of pink is surely not a portion of our feeling of scarlet."

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[1] W. James, *Principles of Psychology*, 1901, I, 546.

4. CORRELATION.

It seemed to be of interest to obtain some information concerning the persistence of individual differences, *i.e.* if a boy made an estimate much higher than the mean value for one disc, whether or not he did so for all the others. It would have taken too long to work out all the possible combinations of correlation coefficients for the various discs, but Table IV gives some sample coefficients calculated by the product moment method from the first set of observations made 13 July.

Table IV also shows the correlation coefficients obtained from the experiments of the second day. They are much lower than those found in the sequence. This may be attributed to the fact that all the discs were of 80 per cent. colour and the standard deviations were high.

It was not convenient to make a correlation between estimates of the same disc made on different days, because the boys present varied slightly from day to day.

Table IV.

Date	Discs	*A* and *B*	*B* and *C*	*C* and *D*	*D* and *E*
1928 13 July	Correlation coefficient	$0\cdot3_2$	$0\cdot6_0$	$0\cdot7_5$	$0\cdot7_1$
	P.E. of correlation	$0\cdot1_1$	$0\cdot0_8$	$0\cdot0_5$	$0\cdot0_6$
	Discs	*F* and *C*	*F* and *F*	*G* and *C*	*H* and *C*
1928 14 July	Correlation coefficient	$0\cdot1_4$*	$0\cdot3_5$	$0\cdot1_6$*	$0\cdot2_4$
		$0\cdot4_7$	—	—	$0\cdot3_1$
	P.E. of correlation	$0\cdot1_2$	$0\ 1_1$	$0\cdot1_2$	$0\cdot1_2$
		$0\cdot1_0$	—	—	$0\cdot1_1$

* These two correlations are both exceptionally low. This is due to a very erratic estimate by one boy of the disc *C*. His two estimates of this disc in this series of observations were 75 and 32! If his values are omitted, the two correlation coefficients both become $0\cdot2_9$.

5. PHOTOMETER MEASUREMENTS.

It was considered desirable to make measurements on the relative reflectivities of the white card and the red blotting paper.

Since it is impossible to make such a comparison in white light, red filters were employed. The wave-length of the red light used was about 6500 Ångström units.

Two sets of experiments were made. For the first series of observations a Richardson aeroplane photometer[1] was adapted.

In the second set of experiments, which were conducted in a darkened room, the distances of the two reflecting surfaces from a lamp were adjusted until the two surfaces reflected equal amounts of light to the observer. The ratio of the reflectivities was then equal to the ratio of the squares of the distances of the surfaces from the lamp.

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[1] Union Géodésique et Géophysique Internationale, Section de Météorologie "Report on photometers for a survey of the reflectivity of the earth's surface," 1928, Camb. Univ. Press.

Let $$R = \frac{\text{reflectivity of white card}}{\text{reflectivity of red blotting paper}}.$$

Then by photometer measurements $R = 1{\cdot}5_6$,
and by distance measurements $R = 1{\cdot}5_8$.

Thus the white card reflects $1{\cdot}5_7$ times as much red light as does the red blotting paper.

6. ACKNOWLEDGMENTS.

The author's thanks are due to all the boys who made the observations; to the Head Master of Manor House School, Mr S. Maxwell, M.A., LL.B., for permission to carry out the experiments in the school; and to the Science Master, Mr T. R. Allitt, B.Sc., both for assistance during the conduct of the experiments in the class-room, and for help in some of the photometric measurements. Also in a special degree the author wishes to express his gratitude to Dr L. F. Richardson, F.R.S., for much helpful advice and criticism throughout the research.

7. SUMMARY.

Quantitative mental estimates of the amount of red present in the pink of certain colour wheels were made by thirty-five boys of average age 15·8 years. The different pinks were produced by varying the percentage of red and white on the colour wheels. If x = percentage amount of red in colour wheel and y = mental estimate of pink, it has been found that the functional relationship between x and y is represented by the hyperbola

$$(x - 156)(y + 56) = -8376.$$

(*Manuscript received* 10 *October* 1928.)

BOUNDARY CONDITIONS FOR CORRELATION COEFFICIENTS

BY P. J. DANIELL.

IN a recent paper[1] Dr J. Ridley Thompson discusses the boundary conditions which must be satisfied (in the case of 3 and also of 4 variables) if certain measures can be regarded as combinations of specific factors together with 'first order group factors'; that is to say, factors each common to 2 variables only. The present paper gives a simpler method of attack which permits an immediate extension to n variables, and to the case of factors each common to p of the n variables. The results in the case of 3 and of 4 variables are the same as Thompson's results.

We also obtain a rough rule that for n measures with factors each common to p of them the *average coefficient of correlation must not exceed* $(p-1)/(n-1)$. Thus if there are 9 variables and their average coefficient of correlation is 0·6 there will be required factors common to at least 6 of them.

Three variables.

Let q_1, q_2, q_3 be the given measures, x_{12} a factor common to q_1, q_2, x_{13} common to q_1, q_3 and x_{23} common to q_2, q_3. In addition y_1, y_2, y_3 are specific factors. All of these variables are taken of unit standard deviation. Then

$$\left.\begin{aligned} q_1 &= a_{12}x_{12} + a_{13}x_{13} + b_1 y_1 \\ q_2 &= a_{21}x_{12} + a_{23}x_{23} + b_2 y_2 \\ q_3 &= a_{31}x_{13} + a_{32}x_{23} + b_3 y_3 \end{aligned}\right\} \qquad \ldots\ldots(1),$$

in which a_{12}, a_{21}, for example, are not to be confused. It can be shown that these are equivalent to those [p. 80 (5)] of Thompson. Since q_1, q_2, q_3 are of unit deviation,

$$\left.\begin{aligned} a_{12}^2 + a_{13}^2 &= 1 - b_1^2 \\ a_{21}^2 + a_{23}^2 &= 1 - b_2^2 \\ a_{31}^2 + a_{32}^2 &= 1 - b_3^2 \end{aligned}\right\} \qquad \ldots\ldots(2),$$

and the correlation coefficients are given by

$$a_{12}a_{21} = r_{12}, \quad a_{13}a_{31} = r_{13}, \quad a_{23}a_{32} = r_{23} \qquad \ldots\ldots(3).$$

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[1] J. R. Thompson, this *Journal*, 1928, XIX, 87, also *ibid*, 1929, XIX, 239.

Our problem is to determine the inequalities which must be satisfied by r_{12}, r_{13}, r_{23} if these 6 equations are to be soluble for the 9 unknowns $a_{12}, a_{21}, \ldots, b_1, b_2, b_3$.

Let t_1, t_2, t_3 be unspecified variables. The quadratic form

$$\begin{aligned} & t_1^2 + t_2^2 + t_3^2 - 2r_{12}t_1t_2 - 2r_{13}t_1t_3 - 2r_{23}t_2t_3 \qquad \ldots\ldots(4) \\ &= (a_{12}^2 + a_{13}^2 + b_1^2)\,t_1^2 + \ldots + \ldots - 2a_{12}a_{21}t_1t_2 - \ldots - \ldots \\ &= b_1^2t_1^2 + b_2^2t_2^2 + b_3^2t_3^2 + (a_{12}t_1 - a_{21}t_2)^2 + (a_{13}t_1 - a_{31}t_3)^2 \\ &\qquad + (a_{23}t_2 - a_{32}t_3)^2, \end{aligned}$$

can never be negative for any real values of t_1, t_2, t_3. Hence, by a well-known property of quadratic forms the determinant

$$\begin{vmatrix} 1 & -r_{12} & -r_{13} \\ -r_{12} & 1 & -r_{23} \\ -r_{13} & -r_{23} & 1 \end{vmatrix} \geqslant 0 \qquad \ldots\ldots(5),$$

that is to say,

$$1 - r_{12}^2 - r_{13}^2 - r_{23}^2 - 2r_{12}r_{13}r_{23} \geqslant 0 \qquad \ldots\ldots(6).$$

This is Thompson's boundary condition for three variables. Furthermore, the equality sign can only arise when it is possible to choose t_1, t_2, t_3 (not all zero) so as to make the quadratic form take on the value 0. This happens only when

$$b_1 = b_2 = b_3 = 0, \quad a_{12}t_1 = a_{21}t_2, \text{ etc.}$$

Multiplying the three last equations together we see that

$$a_{12}a_{23}a_{31} = a_{21}a_{32}a_{13} \qquad \ldots\ldots(7).$$

This is Thompson's equation (10), $ABC = 1$.

It remains to prove the converse that if the boundary condition (6) is satisfied real solutions of equations (2) (3) can be found.

Let t_1, t_2, t_3 be quantities as yet undetermined and choose

$$a_{12} = \sqrt{(r_{12}t_2/t_1)}, \quad a_{21} = \sqrt{(r_{12}t_1/t_2)}, \text{ etc.}$$

Then $a_{12}a_{21} = r_{12}$, etc., and equations (3) are automatically satisfied. Equations (2) now become

$$r_{12}t_2/t_1 + r_{13}t_3/t_1 = 1 - b_1^2, \text{ etc.}$$

These again can be written

$$(1 - b_1^2)\,t_1 - r_{12}t_2 - r_{13}t_3 = 0, \text{ etc.} \qquad \ldots\ldots(8).$$

These three homogeneous equations in t_1, t_2, t_3 can certainly be satisfied (with not all t's equal to 0) if the determinant of the equations is zero, that is, if

$$\begin{vmatrix} 1 - b_1^2 & -r_{12} & -r_{13} \\ -r_{12} & 1 - b_2^2 & -r_{23} \\ -r_{13} & -r_{23} & 1 \end{vmatrix} = 0 \qquad \ldots\ldots(9).$$

This again will be the case if the quadratic form

$$t_1^2 + t_2^2 + t_3^2 - 2r_{12}t_1t_2 - 2r_{13}t_1t_3 - 2r_{23}t_2t_3 - b_1^2t_1^2 - b_2^2t_2^2 - b_3^2t_3^2$$

is never negative but is 0 for a certain choice of the ratios of t_1, t_2, t_3. For this purpose it is sufficient to choose

$$b_1^2 = b_2^2 = b_3^2 = b^2,$$

where b^2 is the minimum value of form (4) subject to the condition

$$t_1^2 + t_2^2 + t_3^2 = 1.$$

Hence, if form (4) is never negative, the problem of finding factors of the kind required is soluble. Sufficient conditions are that

$$1, \quad \begin{vmatrix} 1 & -r_{12} \\ -r_{12} & 1 \end{vmatrix} \quad \text{and} \quad \begin{vmatrix} 1 & -r_{12} & -r_{13} \\ -r_{12} & 1 & -r_{23} \\ -r_{13} & -r_{23} & 1 \end{vmatrix}$$

should all be non-negative. Of these the first two conditions are *always* satisfied so that the third condition is sufficient by itself.

n variables.

This repeats the case of 3 variables and with an obvious notation

$$q_i = \Sigma_j a_{ij} x_{ij} + b_i y_i \qquad \text{......(10).}$$

It follows that

$$\Sigma_j a_{ij}^2 = 1 - b_i^2 \qquad \text{......(11),}$$

$$a_{ij} a_{ji} = r_{ij} \qquad \text{......(12).}$$

The quadratic form

$$\Sigma_i t_i^2 - \Sigma_{i,j} r_{ij} t_i t_j \qquad \text{......(13)}$$

$$= \Sigma_i b_i^2 t_i^2 + \tfrac{1}{2}\Sigma_{i,j} (a_{ij} t_i - a_{ji} t_j)^2 \qquad \text{......(14)}$$

is non-negative for all t_i. [Note that in the summation over all pairs i, j a term like $r_{12}t_1t_2 = r_{21}t_2t_1$ will appear twice.] Then we must have

$$\begin{vmatrix} 1 & -r_{12} & -r_{13} & -r_{14} & \cdots \\ -r_{12} & 1 & -r_{23} & -r_{24} & \cdots \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & 1 \end{vmatrix} \geqslant 0 \qquad \text{......(15).}$$

When we consider the converse, as in the case of 3 variables, it is found that the necessary and sufficient conditions for solubility in terms of first order group factors is that all the determinants,

$$\begin{vmatrix} 1 & -r_{12} & -r_{13} \\ -r_{12} & 1 & -r_{23} \\ -r_{13} & -r_{23} & 1 \end{vmatrix}, \quad \begin{vmatrix} 1 & -r_{12} & -r_{13} & -r_{14} \\ -r_{12} & 1 & -r_{23} & -r_{24} \\ -r_{13} & -r_{23} & 1 & -r_{34} \\ -r_{14} & -r_{24} & -r_{34} & 1 \end{vmatrix}, \quad \ldots.$$

up to the nth order should be non-negative.

The 4th order determinant yields Thompson's condition for 4 variables but in addition 3 variables must also satisfy the 3-variable condition.

n variables, factors common to p at a time.

The n variables are $q_1, q_2, \ldots q_n$ and the specific factors $y_1, y_2, \ldots y_n$ as before. The group factors are called $x_{ijkl\ldots}$. Thus

$$q_i = \frac{1}{(p-1)!} \Sigma_{jkl\ldots} a_{ijkl\ldots} x_{ijkl\ldots} + b_i y_j \qquad \ldots\ldots(16)$$

and there are $p-1$ variable suffixes $jkl\ldots$ which can be interchanged in $(p-1)!$ ways. Then

$$\frac{1}{(p-1)!} \Sigma_{jkl\ldots}\; a^2{}_{ijkl\ldots} = 1 - b_i^2 \qquad \ldots\ldots(17),$$

$$\frac{1}{(p-2)!} \Sigma_{kl\ldots}\; a_{ijkl\ldots} a_{jikl\ldots} = r_{ij} \qquad \ldots\ldots(18),$$

$$\frac{1}{2}\frac{1}{(p-2)!} \Sigma_{ijkl\ldots} (a_{ijkl\ldots} t_i - a_{jikl\ldots} t_j)^2$$

$$= -\Sigma_{ij} r_{ij} t_i t_j + \frac{1}{(p-2)!} \Sigma_{ijkl\ldots} a^2{}_{ijkl\ldots} t_i^2.$$

$$= -\Sigma_{ij} r_{ij} t_i t_j + (p-1)(\Sigma t_i^2 - \Sigma b_i^2 t_i^2).$$

Therefore in this case the quadratic form to be considered is

$$(p-1)\Sigma_i t_i^2 - \Sigma_{ij} r_{ij} t_i t_j \qquad \ldots\ldots(19),$$

and the required condition[1] is that

$$\begin{vmatrix} (p-1) & -r_{12} & -r_{13} & -r_{14} & \cdots \\ -r_{12} & (p-1) & -r_{23} & -r_{24} & \cdots \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & (p-1) \end{vmatrix} \geqslant 0 \ldots\ldots(20),$$

and that the corresponding determinants formed with the first 3, the first 4, the first 5, etc. variables shall all be non-negative.

Approximations.

These formulae are somewhat unwieldy when n is at all large. Moreover, the boundaries will be made blunter by the errors of sampling. Hence an approximation may be of considerable value. In such an approximation it is clear from the conditions that unless p is nearly equal to n the correlations must on the whole be small. For rough purposes we proceed as follows:

Let s be the average of the r's and let

$$r_{ij} = s(1 + \epsilon_{ij}).$$

---

[1] Another form of this determinant is stated in J. R. Thompson, *Proc. Roy. Soc. Edinburgh*, 1928, XLIX, 66.

We shall suppose that s and ϵ are so small that terms of the 5th order in their products are negligible. Then in terms of the 4th order in r we can replace r_{ij} by s, and even in terms of the 3rd order in r we can also do this because of the symmetry of the expressions and because the average of ϵ is zero.

If we now denote the standard deviation of ϵ by δ the determinant in (20) can be expanded in the form

$$(p-1)^n - {}_nC_2\,(p-1)^{n-2}s^2\,(1+\delta^2) - 2\,{}_nC_3\,(p-1)^{n-3}s^3$$
$$+ 3\,{}_nC_4\,(p-1)^{n-4}s^4 - 6\,{}_nC_4\,(p-1)^{n-4}s^4 \geqslant 0.$$

Let
$$\frac{n-1}{p-1}\,s = f,$$
then
$$\frac{n}{n-1}\,\tfrac{1}{2}f^2\,(1+\delta^2) + \frac{n\,(n-2)}{(n-1)^2}\,\tfrac{1}{3}f^3 + \frac{n\,(n-2)\,(n-3)}{(n-1)^3}\,\tfrac{1}{8}f^4 \leqslant 1.$$

When n is large this becomes
$$\tfrac{1}{2}f^2 + \tfrac{1}{3}f^3 + \tfrac{1}{8}f^2\,(f^2 + 4\delta^2) \leqslant 1.$$

If $\delta = 0$, f must be less than 1·05, while if $f = 1$, δ^2 must not exceed $\frac{1}{12}$. Hence it can be taken as a rough rule that $f < 1$ or that
$$\text{average } r < (p-1)/(n-1).$$

In addition to this condition, a similar condition—with the same p—must be satisfied for any number, m, of the variables chosen from the n variables.

As a test of this rough rule we can consider the extreme case of 3 variables as exactly as possible. In this case let us write
$$r_{12} = x, \quad r_{13} = y, \quad r_{23} = z.$$
On the boundary
$$x^2 + y^2 + z^2 + 2xyz = 1,$$
and we wish to find the maximum value of $x + y + z$ when this condition is satisfied. On working it out it is seen that this maximum is obtained when
$$x = y = z = \tfrac{1}{2},$$
and then
$$\text{average } r = \tfrac{1}{2} = (2-1)/(3-1).$$
Hence our rough formula is exactly satisfied in the case of 3 variables. It is not, however, an exact *sufficient* condition for the analysis into first order group factors to be possible.

(*Manuscript received* 12 *September* 1928.)

PUBLICATIONS RECENTLY RECEIVED

Industrial Psychology. Edited by CHARLES S. MYERS. London: Thornton Butterworth (Home University Library). 1929. Pp. 248. 2*s.* 6*d.* net.

For anybody who wants to get a general idea of what industrial psychology is accomplishing in this country this is certainly about the best book available. The introduction, written by Dr Myers, sets forth clearly the purposes, problems and the general basis of industrial psychology. The essays that follow, with the exception of the one by Dr Drever on "The Human Factor in Industrial Relations," are all contributed by investigators of the National Institute of Industrial Psychology. They are uniformly clear, interesting, and represent a great amount of first-hand work. The subjects dealt with are: Work and Environment, by A. M. Hudson Davies; Work and Rest, by Rex Knight; Ease and Speed of Work, by G. H. Miles and A. B. B. Eyre; Unproductive Working Time, by A. Angles; Industrial Accidents, by A. Stephenson; The Measurement of Intelligence and Aptitudes, by F. M. Earle; Choosing a Career, by A. Macrae; Square Pegs and Square Holes, by Winifred Spielman; Industrial Psychology and Welfare Work, by Sheila Bevington; The Economic Aspects of Industrial Psychology, by F. W. Lawe; Industrial Psychology and Agriculture, by W. R. Dunlop.

The Opium Problem. By C. E. TERRY and MILDRED PERRENS. New York. 1928. Pp. xvi + 1042.

This important book is published by the American Committee on Drug Addictions in collaboration with the Bureau of Social Hygiene. Messrs Baillière, Tindall and Cox, of London, are acting as agents for the book in this country. No more comprehensive and authoritative study of the opium problem, in all of its many ramifications, has yet been published, or is likely to appear for a long time. The investigation is naturally mostly concerned with opium addiction in the United States of America, but psychologists, psychiatrists and social workers in every country will find here a mine of useful and significant information. First a survey is made as to the extent of opium taking in America and its increase from early days. Next comes a study of the history of opium from very early days to now. The third chapter deals with etiology, and this is followed by a discussion of the general character of the effects of opium and their relation to both physical and mental disease. A very thorough study of the pathology of opium results occupies two long chapters, the subject being dealt with under the head of Somatic and Psychic changes and the phases of tolerance, dependence and withdrawal being exhaustively considered. Symptomatology next occupies the attention, and this leads to a most interesting survey of the main Types of Users of opium. Various methods of treatment and their comparative values are described, and finally methods of control of the drug: international, national, state and municipal are presented. A very full bibliography and index complete the volume. The thoroughness of the survey should make this a standard book of reference.

Mysteries of the Soul. By RICHARD MULLER FREIENFELS, translated by BERNARD MIALL. London: George Allen and Unwin, Ltd. 1929. Pp. 348. 12*s.* 6*d.* net.

The translator has done his work extraordinarily well, and so far as its English style goes the book makes remarkably good and attractive reading. The actual material is throughout of philosophical and literary rather than of psychological interest. The topics dealt with are: The Infinity of the Soul, The Individual and his

Destiny, A Journey into the Past, The Dramaturgy of Life, The Americanization of the Soul, and The Religion of the Future. From a psychological point of view by far the most interesting of these chapters is the one in which the author tells part of the story of his childhood, though even here he cannot refrain from constant interpretation and comment in accord with the considered views of his later life, and the chapter in which he discusses the "americanization" of life. The latter chapter has many acute observations bearing upon some of the problems of social psychology. On the whole the book is one to be enjoyed rather than one to be studied.

The Science of Character. By LUDWIG KLAGES, translated by W. H. JOHNSON. London: George Allen and Unwin, Ltd. 1929. Pp. 308. 10*s.* 6*d.* net.

This is a work which has enjoyed considerable popularity in Germany, but it is far from easy to see why. No doubt everybody is now turning to a study of temperament and personality for the solution of his practical psychological problems. But the treatment of character in this book is long-winded, woolly and pretentious to a degree, and it is hard to see that the book has any claim to genuine originality of the more theoretical type. The translation is clear, but not particularly pleasant to read. However, the translator had undoubtedly a difficult task.

Colour and Colour Theories. By CHRISTINE LADD-FRANKLIN. London: Kegan Paul. 1929. Pp. xv + 284. 12*s.* 6*d.* net.

The Editor of the International Library of Psychology, Philosophy and Scientific Method has here collected together a very considerable number of Mrs Ladd-Franklin's articles and reviews of books. They are presented with a number of nicely produced colour diagrams most of which are as hypothetical as the theories which they illustrate. Readers may be glad to have these vigorous controversial papers all put together, though the repetition which the method involves is apt to get a bit wearisome.

The Growth of the Mind: an Introduction to Child Psychology. By K. KOFFKA. London: Kegan Paul. 1928. Pp. xix + 427. 15*s.* net.

It is a pleasure to have to record the appearance of a second edition of this stimulating and original book. A good many alterations have been made with a view to rendering the treatment at once more internally consistent and less controversial in tone. Moreover, more recent work of some of the Gestalt psychologists, especially the important researches of Lewin, receives consideration. The book as it stands is undoubtedly one of permanent value, not only for the educationist, but also for every serious student of psychology, and adds distinction to the well-known International Library of Psychology, of which it is one volume.

The Psychology of Philosophers. By ALEXANDER HERZBERG. London: Kegan Paul. 1929. Pp. viii + 228. 10*s.* 6*d.* net.

This volume of the International Library of Psychology discusses the psychological causes which predispose a man to a philosophic outlook. The treatment is in the main in general terms, though the study centres about the lives and work of thirty philosophers who are taken as characteristic. Various special topics are taken up in a series of short and lively chapters, and the author's main thesis is that the great thinkers are men with more than normally intense inhibitions. Many may think the book more than a little superficial, but few will find it uninteresting. The translator, E. B. F. Wareing, has done his work well.

Incentives to Study: A Survey of Student Opinion. By A. B. CRAWFORD. Yale University Press (and Oxford University Press). 1929. Pp. xiii + 194. 23*s*. net.

This is a comprehensive account of the carefully collected opinions of a large number of students at Yale University concerning the motives and incentives that prompt them to carry through their academic work, and the College influences that are most important in shaping their careers. A good many of its findings have, no doubt, a mainly local significance, but the whole study is full of interest for all people who may be concerned with higher education. The book itself is produced with needless magnificence, and is, by consequence, unfortunately expensive.

The Development of the Human Eye. By IDA C. MANN. Cambridge University Press. 1928. Pp. x + 306. 36*s*. net.

There is no other book that covers the story of the development of the human eye with anything like the completeness that is achieved in this extremely valuable volume. As Sir John Parsons says in his introduction: "It has been possible to illustrate every stage of human ocular development by an actual specimen, independent of reference to other doubtfully related species. In this way certain debatable points, resting hitherto on comparative observations only, have been placed on a sounder foundation, and certain other processes for example, those involving growth rates and consequent alteration of position, have been studied for the first time on human material." The book is in fact a splendid record of patient original research, illustrated profusely, and is likely to remain for many years a standard work of reference. A very full and valuable bibliography is included.

The Cytoarchitectonics of the Human Cerebral Cortex. By C. VON ECONOMO, translated by S. PARKER. Oxford University Press. 1929. Pp. xii + 186. 21*s*. net.

This is a clear and excellent translation of Professor von Economo's shorter and recent text-book. It will be of considerable value to anatomists, psychiatrists, some physiologists and here and there to a sufficiently trained psychologist. Those psychologists who are hardly able to follow the work in detail will be particularly interested in the final chapter with its important remarks on the mode of functioning of the cerebral cortex. "Just as the retina is sensitive to light in all its parts and yet needs its totality to produce an accurate picture, so is the intact condition of the whole cerebrum necessary to the execution of perfect conscious psychic functions." Yet in the brain there are centres much like that of the macula lutea in the eye, where certain functions come to a focus and are most potently exercised. There is thus a receptive focus in the posterior, an efferent focus in the anterior, portion of the cerebrum. The author's remarks about these and his speculations concerning their general development are of very great interest.

The Relation of the Skull and Brain to Crime. By W. NORWOOD EAST. Edinburgh: Oliver and Boyd. 1928. 6*d*. Pp. 28. Henderson Trust Lecture, No. VIII.

The general trend of this instructive lecture is historical. An account is given of the work of the phrenologists and of some alienists in the investigation of the skull and brain characteristics of criminals. Contemporary work in psychology, normal and abnormal, is also considered. No very definite conclusions emerge, but the lecturer is most sympathetic to the mode of approach due to Professor Cyril Burt.

Rhythm and Metre. By Thomas Taig. Cardiff: University of Wales Press Board. 1929. Pp. 140. 5*s.* net.

The subject of rhythm has, for obvious reasons, always held an attraction for the psychological experimenter. But most of his work has had little direct concern with the rhythms of word and sound as they appear in poetry, prose and music. The strong modern tendency to try to keep psychological investigation more closely in touch with real life must have led many students to consider a new experimental approach to the study of rhythm less dominated by the idea that all such investigation must attempt to deal only with the simplest and most perfectly controlled instances. For any such students, as well as for any reader whatever who is genuinely interested in rhythm in poetry, prose and music, this will prove an attractive book, raising far more problems than it even attempts to solve, but suggesting many interesting lines of study. The author, having settled his definitions, deals in two largely psychological chapters with Sound Rhythms and The Representation of Speech-Rhythms. The graphical method of representation illustrated in the second of these chapters, though it must be very difficult to write accurately, may appeal to many readers. Thereafter the author deals with: The Pattern of Verse and with The Spoken Line. He presents numerous and apt illustrations, and maintains with much force that the perception of rhythm is dependent upon specific pattern experiences the character of which calls for psychological rather than objective analysis. A final chapter on The History of Music and Verse tries to trace a continuous rhythm development in English metre, and to show how that development has been achieved. The book is a contribution of real value to the psychological study of rhythm.

Introduction to the Study and Interpretation of Drama. By J. W. Kaiser. Amsterdam. 1929. Pp. 77.

"Drama," begins the author, "is a product of a psyche which unconsciously projects its arrangement towards a certain problem into an apparently multi-personal demonstration of life." This seems to mean that in dramatic composition the dramatist reveals much of his own character. From this point of view *Cyrano de Bergerac* and *The Merchant of Venice* are studied in considerable detail.

Modern Language Instruction in Canada. University of Toronto Press. 1928. Vol. I. Pp. xlviii + 547. Vol. II. Pp. 852.

These two large, comprehensive and excellently produced volumes are published by the American and Canadian Committees on Modern Languages, and claim the serious attention of all those who are interested in the teaching of the subjects dealt with. The first volume is chiefly occupied by an annotated bibliography of recent work on language teaching, together with a formulation of many problems still awaiting research. All the way through the emphasis is laid on a psychological method of approach. The second contains a history of modern language teaching in Canada, and a full account of the results of the application on a large scale of tests of language proficiency in Canada, England and America. Not only are the results themselves extremely interesting, but their main significance in regard to needs and methods of future work are set forth clearly and judiciously. The main part of Volume I is published separately under the title: *An Annotated Bibliography of Modern Language Methodology.*

Retention of History in the Sixth, Seventh and Eighth Grades with Special Reference to the Factors that Influence Retention. By Sarah Jane Bassett. Johns Hopkins Studies in Education, No. 17. Baltimore: Johns Hopkins Press. 1928. Pp. viii + 110. $ 1.75.

This is a statistical and experimental study, together with a chapter on the history of work on retention and an annotated bibliography.

Phonophotography in folk music. American negro songs in new notation. By MILTON METFESSEL. The University of North Carolina Press. 1928. Pp. x + 181. $3.0.

The study of folk music by listening to phonographic records is manifestly unsatisfactory. The student tends to concentrate on those features with which his previous training has made him familiar; the unfamiliar tends to be neglected or misinterpreted owing to the elusiveness of the auditory perception. With the advent of phonophotography, by which means the impressions on the records are transformed into tracings on a photographic film, the study enters on a new phase. The idiosyncrasies of folk music, as well as those of the individual singers, are laid bare and can be studied at leisure. Differences of phrasing, of attack, of interpretation may be analysed and compared—a great advantage to those interested in the perceptual side of the psychology of music.

In this volume the methods of obtaining the photographs and of analysing the results are described, while the analysis of portions of many American negro songs are set out in a new "pattern notation," specially devised to bring out the various factors in the singing. Incidentally it is of interest to note that the finished film for a four minutes' record is estimated to cost $25.0, and its analyses an additional $75.0. The volume should be seen by all who are working in this branch of psychology or of aesthetics.

The Terror Dream. By G. H. GREEN, Ph.D., M.A., B.Sc. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1927. Pp. 126. 5*s.* net.

In this essay Dr Green considers the mechanism of the terror dream. According to his analysis the dream is an attempted wish-fulfilment. The reason why terror —a mixture of emotions of which one component is fear—is aroused, is that the object or situation represented in the dream has more than one meaning. Thus, while one tendency or wish is being satisfied, another tendency may be activated by the appreciation of a second symbolical meaning. When the emotion connected with the second meaning is fear, the way is open for the production of a terror dream; or, if the dream is developed further, of an anxiety dream. Then the dream fails to act as the "guardian of sleep."

The essay is a contribution to the literature of dreams.

The Kingdom of the Mind. By JUNE E. DOWNEY. New York: The Macmillan Co. 1927. Pp. xiv + 207. 8*s.* 6*d.* net.

This is the third volume of a series of books on scientific subjects written especially for "young people." The preparation of semi-popular books is always difficult, and the difficulty is perhaps enhanced when the subject matter is psychology, and the prospective readers are boys and girls; but Professor Downey has succeeded and has produced a readable little book which will appeal to many older "boys" and "girls" who take an interest in scientific affairs. From it they will learn a great deal about the functioning of the body and of the mind, and, what is more, how they can experiment on most of the problems for themselves.

Mental Hygiene. By DANIEL WOLFORD LA RUE, Ph.D. New York: The Macmillan Co. 1927. Pp. x + 443. 10*s.* net.

This work which, we are informed, is to carry to Mr La Rue's fellow-students "the working truth about mental hygiene as it appears" to Mr La Rue "after several years' study and teaching of mental hygiene" is divided into four parts: The first part deals with the forces that determine personality, and in it La Rue points out that mental health and disease can both in part be learned. The second part deals with the inheritance and development of traits, the third with mental hygiene of adult life, and the fourth with that of childhood and adolescence. Each chapter is followed by class exercises, problems for further study, topics for special investigation and report, and references to other works. Though a large amount of "phrenomental energy" has probably been expended on its production, the finished product is unsatisfactory. On p. 25 we are informed that a person is a "bio-mental unit." This is amplified and, on p. 28 we reach the conclusion that "Body causes mental changes, and mind causes bodily changes." Later, p. 143, we learn that "the hysteriac...is afflicted with a disease which does not really exist, but is brain-built only.... (p. 144) His weak nervous system is the victim of a stoppage or inhibition at some of its synapses." Presumably La Rue means that mind *may* cause bodily changes and *vice versa*, but why a disease which does not exist should be "brain-built" it is hard to conceive. Presumably, again, he means that there is no sufficient physical cause for the hysterical symptom, that the cause is mental; but a patient with a hysterical paralysis *is* as really diseased as another patient who is suffering from pneumonia. On p. 160 we are informed that "some of the more common disturbances of the neuroses which have also, or may have, a mental aspect, are apoplexy, commonly known as 'shock,' due to disturbance of cerebral circulation which may or may not be caused by syphilis...." Presumably, again, everything in this world, or out of it, "may or may not be caused by syphilis." On p. 360 we learn that "Fear, as an emotion, lies low in the brain." But enough has been said. It is a pity that so much study should have resulted in such loose, inaccurate, and often misleading exposition.

Intelligence Tests: Their Significance for School and Society. By W. F. DEARBORN, M.D., Ph.D. Published by the Houghton Mifflin Company. 1928. Pp. xxiv + 336.

In the course of nine lectures, which are printed in this volume, Professor Dearborn covers many aspects of the theory and practice of intelligence testing. He thinks there is no doubt that the intelligence tests which he discusses do give the teacher indications of the very capacity that he is trying to judge in a much more unsatisfactory way by school examinations. It is, then, a book of special value to the teacher rather than to the psychologist. The book is very interesting, but is possibly too elaborate in conception, while much of the discussion is somewhat redundant.

A Manual of Individual Mental Tests and Testing. By AUGUSTA F. BRONNER, WILLIAM HEALY, GLADYS M. LOWE, and MYRA E. SHIMBERG. Published by Little, Brown, and Company, Boston. 1927. Pp. x + 287.

The earlier chapters of this book are simply a list of mental tests, there being no discussion of the psychological significance or theoretical value of them. In the case of each test the details of methods of scoring, and a table of norms are given, except in the cases of those in which the standards have not been fully worked out. Later in the book some space is devoted to discussion of the claims made for the various tests previously described. It seems that many of the tests referred to might be used in the laboratory, and even for research, in connection with experiments involving introspection; they are, however, presented in this book merely as tests.

Love's Creation. By MARIE CARMICHAEL. London: John Bale, Sons and Danielsson, Ltd. 1928. Pp. 416. 7*s*. 6*d*. net.

This is a novel by Dr Marie Stopes. It makes interesting reading. In it her characters talk, probably as no people ever have talked, or ever will talk, about palaeontology, biology and philosophy. The hero, whose views the reader is clearly expected to approve, believes that a new synthesis of the sciences, based upon the essential unity of larger and larger groups of living elements, is approaching. He endeavours, with the utmost enthusiasm, to expound this belief.

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VOLUME XX JANUARY, 1930 PART 3

SECOND REPORT ON A LAMARCKIAN EXPERIMENT

BY WILLIAM McDOUGALL.

(*From the Psychological Department, Duke University, U.S.A.*)

Section I (pp. 201–207).
  (1) *The earlier experiments and their modifications* (pp. 201–203).
  (2) *Details of the later experiments* (pp. 203–206).
  (3) *Analysis of results of the later experiments* (pp. 206–207).
Section II (pp. 207–213).
  (4) *Objections to the application of the Lamarckian hypothesis* (pp. 207–208).
  (5) *The cross-breeding experiment* (pp. 208–212).
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Section III (pp. 213–217).
  (7) *The nature of the peculiarity induced by the training process* (pp. 213–217).
Summary (pp. 217–218).

SECTION I.

(1) *The earlier experiments and their modifications.*

IN 1927 in the April number of this *Journal* I made a first report on the progress of an experiment for the testing of the Lamarckian hypothesis, describing the methods used and the results obtained in the period 1920 to June 1926. The experiment has been continued and has yielded further results that seem to justify a second report at this time. In the first period two halves of a stock of pure-bred white rats were trained generation by generation in two different tasks. One task consisted in learning to escape from a water-maze; the other in learning to escape from a tank of water by the less brightly illuminated of two gangways. Both procedures seemed to give some evidence of increasing facility in mastering the tasks in successive generations. But the maze-procedure seemed in several respects less satisfactory than the tank-procedure[1]; it has, therefore, been abandoned, and the present report deals only with the continuation of the tank-experiment from 1926 to 1929.

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[1] Especially in that success in the task seemed to be favoured by general vigour, a factor liable to wide variations that cannot be satisfactorily controlled.

During the first period 13 generations of rats were trained in the tank. During the second period 10 more generations (14–23) have been trained; and the increase of facility shown by the later generations is so great as to leave no room for doubt of the objective reality of the superior facility implied by the figures representing the performances of the rats of the trained stock. This peculiar facility implies a constitutional peculiarity induced in the tank-trained stock. (Henceforth the rats of this stock are called Tank-Rats or T.R. and this constitutional peculiarity is referred to as X.) Two questions of great interest arise.

The first and more important question is: Is it possible that X has been acquired by the tank stock through some selective process?

The second question is: Can we define more nearly this factor X?

I shall first report briefly the progress of the experiment and then consider these two questions and the experimental evidence bearing upon them.

The tank-procedure described in the former report has been modified in two respects, neither of which involves any essential modification of the task imposed on the animals. During the first period, owing to lack of time, I did not carry through to completion the training of all the rats of each batch. During the second period, having more time for research work, I have carried the training of every rat through to completion, *i.e.* to the point at which the rat ceases to make any errors and has completely mastered his task. This modification brings two advantages. First, it enables me to state the degree of facility displayed by any rat or group of rats in a simple and satisfactory way, namely, to state the number of errors made by the individual rat in the complete course of training, or the average number of errors made by the rats of any group. Such number of errors seems to be a fairly exact indication of the position of the rat in the scale of facility or of the degree of X possessed by the individual. The average number of errors of the group is a still more satisfactory indication of the constitution of the group in respect of X, especially when supplemented by figures indicating the range of the number of errors within the group.

Secondly, it now appears to me not improbable that the training process preceding the final or critical stage of learning (through which each rat finally graduates to become a finished performer) is of little or no effect; and that the Lamarckian transmission that seems to occur is a function of the achievement of the critical stage of learning.

The second modification of the procedure consists merely in a simplification of the task of the rat in a way that avoids considerable waste of time; namely, a curved sheet of metal is inserted in the tank in such a way as to close the two blind alleys of the tank described in the former report. The tank of the second period has, then, the form indicated in Fig. 1 (cp. Fig. 1 of the former report). The curved line indicates the sheet of metal which shuts off the blind alleys of the further end and confines the rats to the part of the tank nearer the platform *P*.

As in the later part of the first period, the alternating procedure was applied throughout this second period. The rat to be trained is repeatedly placed in the water at *O*. He can escape from the water to the platform *P* only by climbing one of the two gangways *A* and *B*. On each occasion one of the gangways is brightly illuminated from behind and is so wired that the rat, as he emerges from the water on to the gangway, receives a secondary interrupted current through his feet[1]. After each immersion both the shocking and the illuminating current are switched over to the other gangway. Each rat is immersed at *O* 6 times on each day of training (a day occasionally being omitted) until he makes 12 successive escapes without error[2]. The total number of occasions on which the rat has taken the illuminated gangway and received the shock is accepted as the inverse measure of his facility in mastering the task or solving the problem. It is to be noted that each taking of the illuminated gangway is reckoned as one error. The number of errors made by any rat is then the same as the number of shocks required in order to induce unfailing avoidance of the bright gangway.

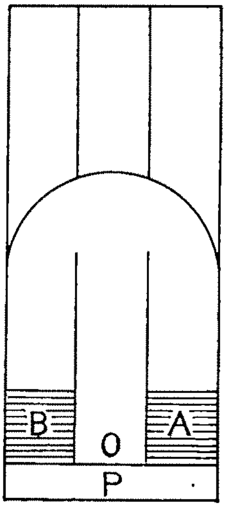

Fig. 1.

(2) *Details of the later experiments.*

Up to June, 1926, rats of 13 generations had been trained in the tank (cp. first report). During the academic year, 1926–7, I was travelling, and my assistant, Mr E. C. Heck, who had already co-operated in the experiment, trained in the tank the 14th, 15th and 16th generations.

Of the 16th generation only 5 rats were trained, and these 5 begat four litters from which 11 rats were taken at random and trained as the 17th generation. Of the 18th generation 22 rats were trained; of the 19th generation 15 rats. Of the 20th generation the training was incomplete owing to unavoidable interruption of work. Of the 21st generation 28 rats were trained; of the 22nd generation 16 rats; and of the 23rd generation 26 rats. In each generation the young for training were chosen at random from all the litters born by the trained animals of the preceding generation, approximately in equal numbers from each litter. From the 14th generation onward, among the rats that entered upon training in each generation, were a small number (not more than 3 of any generation) whose training was not completed, owing in nearly every case to the rat receiving a shock which incapacitated him[1].

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[1] Cp. footnote, p. 204.

[2] Experiment shows that any rat that has attained this level of proficiency will very seldom make another error. Even if tested after an interval of 2 or 3 months, an error is seldom made; and a single error usually then suffices to restore complete proficiency in avoidance of the shock.

The average number of errors of the group of rats of each of these generations is stated in Table I. Although of the 23 rats of the 13th generation 6 rats were not completely trained, it is possible to form a fairly trustworthy estimate of the average number of errors of the whole batch of 23, and I have, therefore, added this to the table[2].

Since the range of facility shown by the rats of each generation is wide, I have added to the table the figures expressing the number of errors made by the best (or most quickly learning) rat and the worst (or most slowly learning) rat of each group.

Table I contains also the corresponding figures for several batches of control rats. Of these the group W.C. 1926 were the progeny of untrained parents newly obtained from the Wistar Institute. They were of the same stock as my original stock and had been (like my own stock) inbred and subjected only to the slight selection involved in rejecting the occasional runt or obviously weakly member of a litter. The training of this group was not carried quite to completion, *i.e.* a few of the 17 rats remained incompletely trained; hence the figure 170 given as representing the average error of this group is approximate only, but it is very improbable that it varies from the true figure by more than five digits more or less.

The group W.C.$_1$ 1927 were 10 rats newly obtained from the Wistar

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[1] This was either a shock through the head which in some cases was immediately fatal, or one which, in some way that remains obscure, paralyzed the hind quarters. Mortality from other causes has been very slight.

[2] The average number of errors of the whole group of 23 rats up to the 30th day of training was 68. At this point 6 rats still made errors. Of the 17th, 18th and 19th generations 5 rats made errors after the 30th day. I have added the number of errors made by these 5 rats to the number made by the 13th generation up to the 30th day. This brings the average error for that generation up to 75. It is highly probable that, if the training had been completed, the average number of errors of the 13th generation would have been rather greater than 75.

Institute and of the same stock as my original stock of 1920 and the controls of 1926.

The group W.C.$_2$ were selected at random from all the litters produced by the group W.C.$_1$ 1927.

The group W.C.$_3$ were selected in the same strictly random fashion from all litters produced by group W.C.$_2$.

These three groups (W.C.$_1$, W.C.$_2$ and W.C.$_3$) were trained with the intention of repeating the early stages of the training process with special attention given to avoidance of any favourable selection. When the Lamarckian effect of training seemed to be clearly shown by W.C.$_2$ and W.C.$_3$, I decided to adopt for later generations a more rigid and sure method of ruling out the possibility of any unwitting favourable selection. Of the 37 rats of group W.C.$_3$, 5 individuals (2 males and 3 females) were very decidedly inferior to all the rest in respect of rate of learning. Whereas the 'average error' of the group was 144 and the errors of the best rat of the group were only 45, the errors made by these 5 rats ranged from 204 to 229, their average being 215. That is to

Table I.

Tank rats		Average no. of errors per rat*	No. of errors of best rat		No. of errors of worst rat
	Rats		Errors	Rats	
13th gen.	23	68 + (probably 75 +)	30	1	?
14th ,,	10	80	42	1	102
15th ,,	10	70	39	1	96
16th ,,	5	73	39	1	88
17th ,,	11	46	9	1	147
18th ,,	22	62	15	1	142
19th ,,	15	47	12	1	100
21st ,,	34	37	9	3	74
22nd ,,	16	36	6	3	89
23rd ,,	26	25	3	2	71
Control rats (unselected):					
W.C. 1926	17	170†	132	2	?
W.C.$_1$ 1927	10	164	90	1	299
W.C.$_2$	22	149	66	3	234
W.C.$_3$	37	144	45	2	229
W.C.$_4$	10	114	42	1	170
Control rats (adversely selected):					
W.C.$_3$	5	215	204	2	229
W.C.$_4$ (W.H.)	18	166	75	2	241
W.C.$_5$ (W.H.)	11	134	81	3	196

* All figures of this column state nearest whole number.

† The training of this group (W.C. 1926) was carried only to the 45th day, when 10 rats still made errors. The average number of errors actually made up to the 45th day was 137. The figure 170 is a conservative estimate reached by adding to the total of errors actually made up to the 45th day, the number of errors made by the 10 rats of the group W.C. 1927 after their 45th day of training.

say, these 5 rats were, in respect of facility in this task, a nearly homogeneous group of very low facility; they were decidedly slower to learn than the average rat of untrained stock. I took these 5 rats as the ancestors of the later generations to be trained. I trained all the three litters produced by these 3 slow females crossed with the 2 equally slow males. These three litters are the 18 rats of group W.C.$_4$ (adversely selected).

These 18 rats were trained in three batches each of 6 rats. From each of the three batches the 3 best performers were set apart as Stock W.C.$_5$ (B.H.) (*i.e.* Wistar controls—better half). The worse half of each batch were then interbred and from their several litters 12 rats were taken at random to form group W.C.$_5$ (W.H.). The group W.C.$_5$ (W.H.) thus is the product of a severely adverse selection exercised on the two preceding generations, W.C.$_3$ and W.C.$_4$. It is my intention to continue for some further generations the process of breeding from the worse half of each group trained and to train from among the progeny of this worse half a group selected in strictly random fashion. I can think of no surer method of excluding favourable selection.

(3) *Analysis of results of the later experiments.*

The main features of interest of Table I are the following:

(1) Although, unfortunately, I have no record of the average of T.R. 1st generation, it is a fair presumption that its 'average error' would not have been widely different from that of the groups W.C. 1926 and W.C.$_1$, both of which are rats of the same Wistar Institute stock whose ancestors have undergone no training. The average error of T.R. 1st generation could hardly have been less than 150; in all probability it would have been about 165 or a little higher. The generations 13 to 23 show very marked increase of facility as compared with the controls of untrained parentage (W.C. 1926 and W.C.$_1$) and with the presumptive facility of T.R. 1st generation; the facility reaching a maximum in T.R. 23rd generation, whose 'average error' is only 25.

(2) This superior facility of the generations 13 to 23 is revealed not only by the diminution of the average error (from the neighbourhood of 150 to 25 in generation 23) but also by the facts indicated in the last two columns. In the 23rd generation 3 rats have such facility in learning that they make only three errors apiece, *i.e.* have learnt to avoid the bright gangway entirely after only three shocks from it; whereas the rat of greatest facility of the untrained control stock makes 90 errors, or requires 90 repetitions of the shock, before he learns to avoid

entirely the bright gangway. Again, all the slowest rats of the generations 13 to 23 are very much superior to the slowest of the untrained stock. And the slowest rat of T.R. generation 23 is decidedly superior (71 errors) to the best rat of the untrained stock (90 errors). *There is thus no overlap* between T.R. generation 23 and the untrained stock; every member of T.R. generation 23 is superior to the best of the untrained stock, in spite of the very wide range of the facility in all groups. The same is true of T.R. generations 21 and 22.

(3) There is fairly steady improvement of the T.R.'s from generation 13 to generation 23, shown in all three columns of the table.

(4) There is similar improvement shown in all three columns from W.C.$_1$ to W.C.$_4$ of the unselected controls.

(5) The very severely adverse selection applied in producing the group W.C.$_4$ throws back this group so that it is inferior in facility to W.C.$_1$ (the 5 parents of group W.C.$_4$ gave 'average error' of 215).

(6) In spite of the further adverse selection applied to produce group W.C.$_5$, this group shows advance on its parents and is superior in all three columns not only to the untrained stock (W.C. 1926 and W.C.$_1$, 1927) but also to W.C.$_2$, and is superior in columns 1 and 3 to W.C.$_3$. This seems to imply that the Lamarckian transmission of acquired facility overcomes the opposed influence of strongly adverse selection. This would seem to be a most crucial experiment; and it will, I hope, be continued through a number of generations. The large advance of group W.C.$_4$ upon their parents may be regarded as, in part at least, a phenomenon of regression towards the mean. But this explanation will not hold for the further advance of group W.C.$_5$, since this advance carries the group beyond the mean of the grandparental performance.

SECTION II.

(4) *Objections to the application of the Lamarckian hypothesis.*

In this section I propose to discuss certain possibilities of explaining away the results shown in Table I as having been produced in some other way than by Lamarckian transmission, some of which have been suggested by critics of my former report, to whom I return thanks.

(1) Can it be that the superiority of the trained generations is due to some direct influence of the trained parents upon the offspring either during intrauterine life or in the period between birth and the end of training?

It should here be said that the training of each batch of rats was begun in nearly all cases about the end of the 4th week and that the mother was allowed to remain with the young up to about the end of the 5th or 6th week. The superiority of the young T.R.'s to the controls might then be due (*a*) to the transmission to the embryo of some hormone (*e.g.* a fear-exciting hormone), (*b*) to actual instruction or warning given by the mother, (*c*) to timidity or other emotional state induced in the mother by the training-process and imparted in some degree to her progeny by sympathetic induction, (*d*) to some 'telepathic' impression from mother to young.

(2) Can it be that rats of the trained stock are unduly favoured by me in the course of training; that I, being naturally interested in, or desirous of finding, an increase of facility in the successive generations, have, in various subtle ways not easily to be defined, favoured the trained stock?[1]

(3) Can it be that all the differences of rate of learning throughout the very wide range (expressed by 3 errors of the best T.R.'s and the 299 errors of the worst W.C.'s) are not expressions of genetic or constitutional differences, but are purely or in the main due to 'accidental' influences exerted on the rats before and during training? This last, I suppose, is the view to which the extreme behaviourist would incline.

(4) Can it be that, in spite of my efforts to avoid it, selection favourable to the increase of *X* has been exercised to an extent capable of producing the superior facility of the later generations?

There are, I submit, ample grounds for ruling out all these vague possibilities. The steady increase of the induced facility throughout the generations answers these four questions in the negative.

And this answer to all these questions is strongly supported by the following experiment.

(5) *The cross-breeding experiment.*

The 3 very slow mothers of W.C.$_3$ (each of which made about 215 errors) were mated with the 2 slowest males of the same group (each of which made about 210 errors). The 18 progeny resulting from this mating gave an 'average error' of 166. The same 3 mothers were then mated with 3 males of the T.R. 21st generation, each of which had made during training approximately 40 errors.

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[1] In this connection the vague possibility of some 'telepathic' influence from me to the rat must be regarded seriously in view of Bechterew's claim to have demonstrated telepathy between man and dog, and Dr Rhine's seemingly successful telepathic experiments with a horse.

From these crosses (between the females of slightly trained stock and very low facility and males of much trained ancestry and high facility) there resulted three litters of 16 rats. These 16 cross-breds gave an 'average error' of 62 (and the average errors of the three litters were nearly identical, namely 65, 65, 56), and a range of errors from 8 for the best rat to 126 for the worst.

After training, these 16 cross-breds were interbred, giving a large number of litters of F_2 generation. From these many F_2 young, 12 were selected at random and trained. They gave 'average error' of 64 and ranged from 39–114. That is to say, the F_2 generation showed almost exactly the same facility as the F_1 generation; and both showed very strongly the influence of the fathers of highly trained stock and high facility.

These cross-breeding results leave little room for doubt that the degree of facility displayed by an animal, and still more the average facility of a group, is a function in the main of some factor or factors X in the genetic constitution of the animals, and that this factor is transmitted by both the male and the female. In these cases the father came into no kind of relation with his offspring other than through his sperm cells. The experiment, then, goes far to rule out all explanation through parental influences other than through the germ cells.

Further evidence bearing on question (3), and tending to show that the facility displayed by any group is in the main a function of its genetic constitution, is the following. We have seen that the 5 slowest rats of group W.C.$_3$ (av. er. = 215) produced slow offspring (av. er. = 166). Of this same group W.C.$_3$, 4 rats, all of which gave about 130 errors, were interbred and their progeny of 10 rats gave 'average error' of 114.

Again, while the worse half of the group W.C.$_4$ produced progeny whose 'average error' was 134; the better half of the same group produced progeny whose 'average error' was 70. These are only a few special instances illustrating the general rule which holds throughout, namely, that, in respect of this particular facility, the offspring show pretty close resemblance to their parents, a resemblance which is the closer the larger the groups of parents and offspring compared.

I turn now to consider in more detail the most insidious possibility of error, namely, that suggested in question (2). There are, I submit, sufficient grounds for a confident negative answer to this question.

First, the different rates of learning of the rats of different groups is not the only evidence of a constitutional difference between them. In the later T.R. generations it became more and more obvious that the behaviour in the tank of the rats of trained stock distinctly differs from that of rats of untrained or only slightly trained stock. The difference can best be expressed by saying that the behaviour of the rats of highly-trained stock is markedly more tentative, hesitating, cautious, or exploratory than that of the other rats. Whereas the control rats in the early part of their training loiter very little, hesitate very little, and, having emerged into either lateral passage of the tank, rush, almost without exception, straight at and on to the gangway; the rats of the later T.R. generations are apt to loiter long near *O*; when they emerge from *O* they are apt to hesitate and oscillate between the two lateral passages; and, having entered one passage, they will frequently turn back and enter the other, in many cases repeating this turning back many times. Even when they approach closely to either the dim or the bright gangway they may turn back from it; and not infrequently they approach cautiously the bright gangway, touch it lightly with nose or paw, receive a slight shock and forthwith retreat. I have never observed such cautious or tentative behaviour in any rat of the control stock at an early stage of training. It is true that the rats of the control stock eventually, as they approach the critical point of their training, begin to exhibit something of this tentative or cautious behaviour, loitering, hesitating, oscillating and rarely turning back after having approached the bright gangway. It seems, then, that the rats of the later T.R. generations develop spontaneously (or in response to a very few shocks) a hesitancy and caution which is induced in the control rats only by a long course of training involving many repetitions of the shocks; or loosely, the rats of T.R. 23 are born in that condition which the control rats reach only after much training. So marked is this difference that either Dr Rhine or myself could generally distinguish pretty confidently rats of the two stocks merely on seeing them immersed in the tank for a few seconds at an early stage of training[1].

But perhaps the best negation to question (2) is afforded by the following facts. In training the generations W.C.$_2$, W.C.$_3$, W.C.$_4$ and W.C.$_5$, I was just as keen to see them make a good record as I was in training the later T.R. generations; and these generations of the two stocks were trained under exactly similar conditions, on the same days and in immediate succession, often in alternating batches. If, then, my desire to see a batch of rats make a good record could in any subtle way lead me to favour that batch, it should have worked equally favourably on the W.C.$_2$, W.C.$_3$, W.C.$_4$ and W.C.$_5$ generations, as on the later T.R. generations. But the difference between the 'average errors' of the two stocks shows that if, in spite of the best intention to be impartial in my handling, I do not altogether succeed in avoiding partiality, this is an influence of very minor importance which can neither account for nor disguise the very large differences of constitution between the two stocks.

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[1] During the academic year 1928–9 I have had the co-operation of Dr J. B. Rhine, a well trained biologist, a most careful worker. I have to thank Dr Rhine most heartily for his assistance and also the President of Duke University who made it possible.

In this connection I make special report on the intensity and duration of the shock delivered to each rat as it takes the bright gangway. For it is in this respect that the danger of partiality is most insidious.

It is obviously desirable that in these respects the shocks delivered to the rats should be as nearly as possible uniform. Complete standardization is impossible, since the intensity of the shock received must vary not only with the strength of the primary current, but also to some extent with the somewhat variable behaviour of the interruptor and with the kind of contact made by the rat. The principle I have followed is to adjust the secondary coil at such a distance from the primary that the rat's limb-muscles are tetanized if he makes good contact. If under these conditions a rat rushes up the gangway with great energy he may escape tetanization, though, as shown by his squeak and other details of behaviour, he does not fail to receive the shock. If the rat is tetanized, he is held during 3 seconds; when the current is broken and he runs up the gangway. Since, then, standardization of the intensity and duration of the shock is very imperfect, it is of some importance to know whether and to what degree different degrees of shock conduce differently to rapidity of learning.

Dr Rhine has therefore made for me the following experiment. He used shocks of three degrees of intensity, light (just sufficient to make the rat squeak as he passed up the gangway) medium (almost sufficient to tetanize the limbs, *i.e.* would tetanize if the rat made exceptionally good contact) and heavy (strong enough to tetanize in almost every instance); the tetanization being continued for 3 seconds.

Three litters of W.C.$_4$ generation (offspring of the unselected W.C.$_3$ group) were split into three groups of 6, 2 rats from each litter; and one group was trained under each strength of shock up to the 82nd day, when the experiment had to be discontinued. Under the light shock no rats learnt; under the medium shock 1 learnt on the 54th day, 1 on the 71st; the other 4 failed to learn. Under the heavy shock 1 learnt on the 52nd day, 1 on the 57th and 1 on the 59th day; the other 3 failed to learn.

He repeated this experiment with groups of rats of T.R. generation 23; making up each group of 4 rats by splitting litters between the groups. Of these the 4 rats under light shock learnt in 24, 26, 27 and 29 days. Those under medium shock learnt in 17, 19, 19 and 21 days. Those under heavy shock learnt in 8, 9, 10 and 14 days.

This experiment seems to show pretty definitely that the heavier the shock up to the point of tetanization the more favourable is it to rapid learning; though the difference between the effects of medium and heavy shock is not very marked.

Since all my shocks fell undoubtedly within the range of the medium to heavy shocks of Dr Rhine's experiment, it follows that there was not wide scope for unwitting partiality on my part in the administration of the shocks, no such partiality as could account for more than a small fraction of the differences of facility revealed in the training process.

But I have given the foregoing account of this experiment of Dr Rhine's with a second object in view; namely, the results of the experiment, conducted by this independent and very careful worker, bring out very clearly the large superiority of facility of the T.R. 23rd generation to the W.C.$_4$ generation—the difference revealed in his experiment with shocks of three different strengths corresponds pretty well with the difference brought out by my training of groups of these two generations (namely, W.C.$_4$ gives av. er. 114 while T.R. 23 gives av. er. 25). The difference found by Dr Rhine is in fact rather greater than that found by me in equivalent groups. The average error made by Dr Rhine's T.R. 23 group under heavy shock was 22—thus a little better than my finding. While of his W.C.$_4$ group (though the training was not completed) the average error under heavy shock cannot have been less than 165 (thus rather worse than my finding).

In my view this confirmation by Dr Rhine (which is only one of several similar instances) of my findings leaves no room to doubt that the differences of facility revealed by my training procedure are real and objective facts and are not due, in any appreciable measure, to defects of my manipulations.

(6) *Selection and improvement.*

Lastly, a few words about the possibilities of selection having produced, or favoured the production of, this change of constitution implied by the greatly increased facility of the later generations.

My method of selection of T.R.'s for training in each generation is to make up batches of 6 rats by taking at random an equal number of rats from each of several litters of approximately the same age; and to secure that all litters born of the parent generation (after training) are represented by about equal numbers. Thus if ten litters are born of the parent generation, I take at random (rejecting only the occasional obvious weakling) 2 rats from each litter and place them in the group to be trained. Of course in any particular generation this process might fortuitously result in a favourable selection. But it is just as likely to result in an unfavourable selection. And in the course of many generations, the chances against any large predominance of favourable selections must be very great.

It is unfortunate that, of the 16th generation T.R., 5 rats only were trained owing to the pressure on Mr Heck's time. So small a number gives scope for a markedly favourable fortuitous selection. But that the selection was not markedly favourable is shown by the relatively high 'average error' of the group (73) as well as by their range of error.

In this connection I insist again on the advance shown by the generations W.C.$_4$, W.C.$_5$, in spite of strongly adverse selection.

SECTION III.

(7) *The nature of the peculiarity induced by the training process.*

In order to establish the reality of Lamarckian transmission it is not necessary to be able to define exactly the nature of that which is transmitted. It suffices to show that the behaviour of successive generations is modified in the direction of the behaviour induced by training of the parents, or, as in the present experiment, to show that, in successive generations of the trained stock, facility in acquiring a certain mode of behaviour increases generation after generation. I know that in face of such facts as are presented in the foregoing pages some of the neo-Darwinians will resort to dialectics. They will assert that there is no essential difference between white and black since both are but shades of grey; that whiteness and blackness are but varying subjective interpretations of one phenomenon; that in fact all cats are grey. I incline to believe that the difference between white and black is real, and that the difference between modification of a stock by selection on the one hand and by transmission of acquired modifications on the other is equally real, and is not to be conjured away by any verbal jugglery, *e.g.* by pointing out that in reality no characters are transmitted but only potentialities of development or of reactions to environmental stimuli.

It would close the door to some of this verbal jugglery if we could define more nearly that X which seems to be transmitted as the innate basis of the superior facility of the trained stock. Dr Rhine is directing his experiments largely to this question, and I hope that in due course he will publish an article that will throw considerable light upon it. In the present article I will indicate only very briefly some possibilities and some light on them which we have already obtained. Reminding the reader that by X I mean that in the innate constitution of the rat under training which gives the T.R.'s of later generations marked superiority to the control rats and to T.R.'s of earlier generations in respect of facility in learning to escape from the tank by the dim rather than the bright gangway, I state and partially and provisionally answer certain questions without describing the experimental evidence on which the answers are founded. This evidence will, I hope, be set forth in a later article by Dr Rhine.

(1) Is X merely a lower degree of vitality or vigour which predisposes the T.R.'s to go slowly and tentatively about their task?

There is no evidence that the T.R. stock is less vigorous than the control stock. And several litters of control stock intentionally brought up to be less vigorous than normal litters (by leaving very large litters of 12 or more young to be suckled by the one mother) have shown no superiority in learning to the normal litters.

(2) Is X an increase of 'general intelligence,' of Spearman's G factor? This interpretation is suggested by the fact that the rats of the stock trained for some generations in the water-maze showed in the tank task a facility markedly greater than that of the control rats[1].

We have made experiments which seem to show clearly that this cannot be a main or important part of X.

(3) Is X an abnormally great general timidity? Such timidity might either be transmitted through the germ-cells or by chemical diffusion (of a hypothetical fear-hormone) from the blood of the mother.

The cross-breeding experiment (p. 208) seems to rule out the latter alternative. It is further negatived by the fact that a few W.C. mothers were trained up to the day before parturition, and their progeny showed no excess of facility over other litters of W.C.'s of the same generation. The former alternative is shown to be of no great importance, if any, by the fact that the T.R.'s show to those long familiar with them no signs of abnormal general timidity; whereas some of the very large litters of W.C.'s were not only lacking in vigour and undersized, but showed so much general timidity that it obtruded itself on the notice of casual observers; yet showed no superior facility.

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[1] Cp. first report in this *Journal.*

(4) Is X a specific fear of the bright gangway, or a special liability to fear in the particular circumstances of immersion in the tank? This seems to be equivalent to asking—Is X a form of 'conditioned-reflex' in the sense in which Pavlov uses that term?

The hesitant, cautious behaviour of the T.R.'s in the tank lends colour to this view. It appears that every rat, before it learns to avoid the bright gangway, must attain this stage of hesitant cautious behaviour in the tank. In this stage of hesitant behaviour it would seem that the rat is the seat of a real conflict of impulses; of the impulse to rush to the gangway so as to escape from the water and of the impulse to retreat from the gangway. Such hesitation, such conflict of opposed impulses, is the beginning of wisdom, is the necessary pre-condition of a discriminatory judgment. The behaviour of the rats in this stage of learning implies very clearly such conflict of impulses.

It is an interesting question—How does the repetition of the shock on the bright gangway eventually bring every rat to effect discrimination, avoidance of the bright and choosing of the dim gangway? A rat may suffer the shock many (say 200) times, and still rush headlong at both gangways, without the least sign of hesitation or effort to discriminate. Yet, after further repetitions, he begins to hesitate, to turn from one to the other, and then, after a shorter or longer period (usually extending through some 2 or 3 days of training) of such hesitation, achieves the discrimination. It seems natural to suppose that the successive shocks produce a cumulative effect, which, although insufficient at first to modify behaviour overtly, does so modify it in the direction of hesitation when the effect reaches a certain degree or level. Yet an alternative possibility suggests itself. Perhaps there is no such cumulative effect. Perhaps there is in the brain some barrier that must be broken down before the experience of the shock can lead to hesitation. If so, then the rat which hesitates after a few shocks may differ from the rat which begins to hesitate only after many shocks in that this barrier in the former is less tough (is perhaps of lower synaptic resistance) than in the latter. And it may be that the breaking down of the barrier requires not only the shock but also some favourable conjunction of conditions. The rat with the weaker barrier will then need for the breaking of it a less favourable conjunction (a constellation of fewer favouring conditions) than is required by the rat with the stronger barrier. If so, then the former rat runs a better chance of meeting with the required conjunction at an early period of its training. Hence, on the average the weaker a rat's barrier, the earlier in the course of training will it begin to hesitate.

It would seem that any impulse of avoidance evoked by the sight of the bright gangway cannot, in the first period of training, contend successfully against the strong impulse to escape as rapidly as possible from the water by the visually presented route. But in the second period of training, the period of hesitation, the impulse to retreat from or to avoid the gangway enters into effective conflict with the impulse to rush up the gangway. But such conflict of impulses, though a necessary condition of success, is not enough. Something more is needed, namely, the effectuation of a discriminatory judgment. At first both gangways evoke the impulse of retreat, and the rat oscillates, looks first at one, then turns about and looks at the other, in some cases approaching nearly to each gangway again and again. Among the T.R.'s of later generations occur individuals which show so strong an aversion from both gangways that they refuse to ascend either; on some occasions of immersion they would drown if I did not take them from the water. That is to say, these individuals quickly enter the stage of effective conflict of impulses; but they are slow to achieve discrimination. The phase of hesitation seems to be terminated more or less abruptly by a definite act of discrimination, constituting a critical point in the process of learning. This is implied by the changed behaviour of the rat. It is not that in this phase he never enters the brightly lit passage or approaches the bright gangway; it is rather that, if he does so, he turns about and swims swiftly and directly to the other gangway and so out.

(5) Is it possible that *X* consists in part or wholly of a toleration for, or diminished aversion from, the water, such that immersion evokes a less strong impulse to escape from the water? Such diminution of aversion would permit the impulse of retreat from the gangway to enter more readily (at an earlier stage) into effective conflict with the impulse to escape.

Direct observation of the rats when first immersed reveals no such diminished aversion. The T.R.'s of later generations seem at first as urgent to escape from the water as the controls; though there may be a slight difference in this respect. It is only after receiving one or more shocks on the gangway that the hesitant behaviour clearly appears. The time spent in the water before escaping is then very much prolonged as compared with the time spent in the water before the hesitant phase sets in. Before the beginning of the hesitant phase the time spent in the water on each immersion is little more in all cases than the minimum time required for escape; in the hesitant phase it is commonly prolonged to one or two minutes, and in many cases is still longer.

There would seem, then, to be two main factors making for success or rapidity of learning: first, the readiness with which the impulse to retreat from the bright gangway enters into effective conflict with the impulse to escape from the water; and this may depend either upon the relative strengths of the two impulses, or upon some inner conditions that facilitate such conflict or render it difficult; secondly, facility in effecting discrimination between the bright and the dim gangways after the hesitant phase has set in. The rats that learn most quickly would seem to be those in which both these factors are favourable.

At present I incline to the view that X comprises as its chief constituents both of these factors. We may, I think, fairly suppose that the discriminatory act brings into play a high level of cortical function, one which the rat's ordinary life in the cage hardly at all brings into play. It may be, then, that this functioning of a cortical level hitherto latent in the race is a principal factor of the transmitted X. The peculiar situation in which the rat finds itself in the tank may be supposed to force into activity this hitherto latent or unused level of potential cortical function. Something of this sort would seem to occur in all successful educational processes, whether in animals or in men.

SUMMARY.

Twenty-three generations of rats have been trained in the tank to the performance of a specific task. The rats of the successive generations have displayed increasing facility in mastering this task. Whereas rats of the control stocks make on the average about 165 errors (and receive the same number of shocks) before learning to avoid the shock, rats of the 23rd generation of trained stock make on the average only 25 errors; the latter have acquired a greatly increased facility in mastering the task, the increase being measured by the difference between 165 and 25 shocks required for learning.

The average degree of facility shown by any group of rats is in the main a function of their genetic constitution.

In the light of our present knowledge there would seem to be only two ways in which such change of constitution as is shown by the rats of the trained stock can be brought about: first, by steady selection of such variations or mutations as may occur in the direction of such change; secondly, by transmission of modifications acquired by the rats in the course of training.

It seems very improbable that the former process, selection, can have played any appreciable part in producing the change of constitution here described; and still more improbable that selection can have been the main or the sole process: first, because selection (other than strictly random selection) was carefully avoided by me from first to last in the conduct of the experiment upon the T.R.'s; secondly, because in the stock W.C.$_1$ to W.C.$_5$ a selection strongly adverse to the increase of facility was exercised on the generations W.C.$_3$ and W.C.$_4$, and, nevertheless, marked increase of facility was shown by the generation W.C.$_5$.

It begins to look to me as though Lamarckian transmission were a real process in nature; and I submit for criticism the proposition that, if continuance of the experiment, combining training with strongly adverse selection, should result in steadily increasing facility, the reality of Lamarckian transmission will have been demonstrated.

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THE TECHNIQUE OF EXPERIMENTATION ON THE PSYCHO-GALVANIC REFLEX PHENOMENON AND THE PHENOMENON OF TARCHANOFF. I.

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I. *The physical nature of the phenomena* (pp. 219–228).
II. *The measurement of the psycho-galvanic reflex* (pp. 228–239).

I. THE PHYSICAL NATURE OF THE PHENOMENA.

Two distinct changes of an electrical nature can be measured in a human subject as accompaniments to mental processes or as responses to external stimuli. The psycho-galvanic reflex (P.G.R.) is an increase in the current passing through the body from an external source. The phenomenon of Tarchanoff (T-phenomenon) is a somewhat more complex change in the current which passes between two electrodes on the skin when no external source of current is used.

That the T-phenomenon is a change in the potential difference between the two points of the skin on which the electrodes are placed, there can be no doubt. About the physical nature of the P.G.R. there is more dispute. Gildemeister believes that the application of an external E.M.F. to the skin causes the production of a back E.M.F. of polarization which is comparable in size with the imposed E.M.F. itself, and that the P.G.R. is a decrease in this polarization (1, 2). Einthoven and Bijtel believe, on the other hand, that the part played by polarization in the skin is much smaller than Gildemeister has maintained, and that the true resistance of the skin is relatively large (3). If they are right, there is no reason for supposing that the P.G.R. is anything but a change in skin resistance. There is no simple way of deciding between these alternatives[1]. In a direct current circuit, a decrease of polarization is indistinguishable from a decrease of resistance. Evidence on the matter can be obtained by the use of high frequency alternating currents and also by direct currents of high E.M.F., but the results are difficult of interpretation. In a prolonged experimental investigation, I have convinced myself that Gildemeister's explanation is the true one. It will, therefore, be assumed to be true for the remainder of the present article.

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[1] It is generally stated by recent writers on the subject that the polarization theory is proved by the fact that Gildemeister showed that when alternating currents are used the resistance of the body is much lower and the P.G.R. is absent (1). This, however, does not settle the matter. Einthoven and Bijtel put forward an alternative theory of the lowering of body resistance to alternating current (3), and Gildemeister's evidence for the polarization theory depends on the use of a much more complex criterion (2). Moreover, even if the P.G.R. is a phenomenon of reduced polarizability, it ought to be present with alternating current although much reduced in amount. In an investigation not yet published, I have found that this expectation is fulfilled.

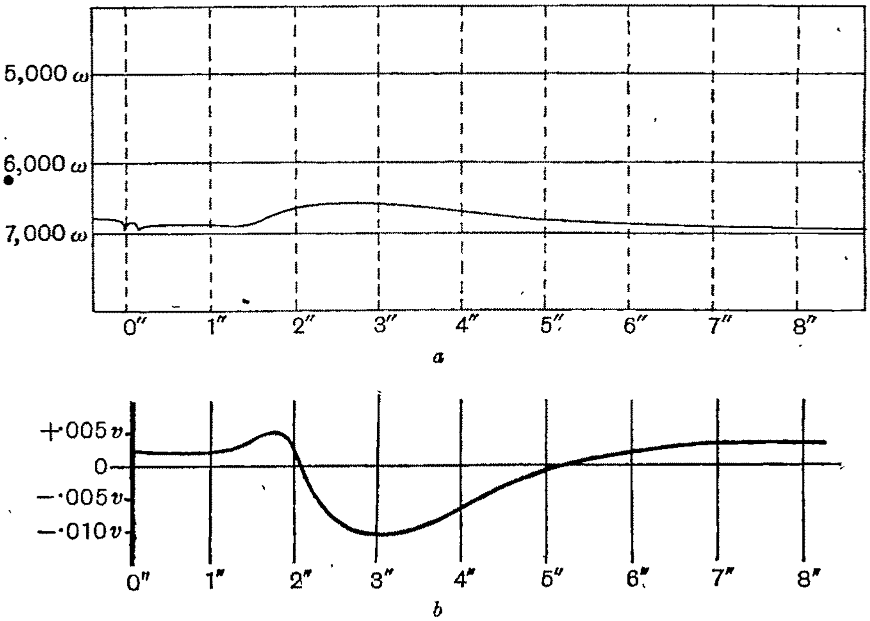

Fig. 1.

a. Photographic record of change in apparent resistance of a subject for 8 seconds after discharge of toy pistol (measured by string galvanometer in a Wheatstone Bridge circuit).

b. Drawing from record of change in somatic E.M.F. of the same subject to the same stimulus in the same series of experiments (showing threefold change in E.M.F. and complete reversal of current during negative change).

The typical form of the P.G.R. phenomenon is shown in Fig. 1 *a*. This was obtained on a string galvanometer in a Wheatstone bridge circuit with an E.M.F. of 1·86 volts between electrodes on the back and palm of the hand (back positive). The stimulus used was the report of a toy pistol. There is a slight increase of current taking place in this particular record in two stages, one at the moment of stimulation and one between 1/10th and 1/5th sec., amounting in all to an increase in apparent resistance of about 100 ohms. This positive change is generally attributed to the electrical effects of muscular contractions. With stimuli less startling than the pistol it is generally absent, although it is sometimes found in the P.G.R. response to any stimulation. From this positive change there is incomplete recovery and the current becomes constant until 1·4 sec. after stimulation when the apparent resistance falls by about 400 ohms to a minimum at 2·8 sec., followed by a recovery which is complete in between 7 and 8 sec. In many hundreds of records with varying stimuli, this curve of decrease in apparent resistance is reproduced without any variation in general form noticeable on casual examination[1], although the latent period is somewhat variable and the time of maximum deflection is more so. The most marked variation of the P.G.R. is the common absence of the initial positive deflection.

One typical form of the phenomenon of Tarchanoff is shown in Fig. 1 *b*. This was reproduced from a record with a string galvanometer obtained at the same sitting as Fig. 1 *a*. No external E.M.F. was used and the current was taken through an external resistance of 10,000 ohms[2]. A deflection above the zero line indicates a direction of current in which the back of the hand is positive and the palm negative (since this is the almost invariable rule, this direction of flow is marked as positive, the other as negative). The initial potential difference is seen to be about 2 mv. After a latent period of 1·2 sec., it rises to 5 mv., then falling to − 10 mv. at 3·1 sec., afterwards rising slowly to 4 mv. at 8 sec., falling again slowly to its initial value. The end of the reaction is not shown on this diagram. The recovery is noticeably slower than that of the P.G.R., and may not be complete until about 15 sec. after the stimulation. The latent periods of reactions and the maximum and minimum points are found to be somewhat variable, even when the experiment is carried out under constant conditions. The latent periods have been shown by Gildemeister and Ebbinghaus to depend on the skin temperature (5).

The T-phenomenon is much more variable than the P.G.R. The most striking variation is the replacement of the three-fold reaction shown in Fig. 1 *b* by a simple increase in the current. This form of the reaction is shown in Fig. 2 *A* (2). It can often be seen that, although there is no

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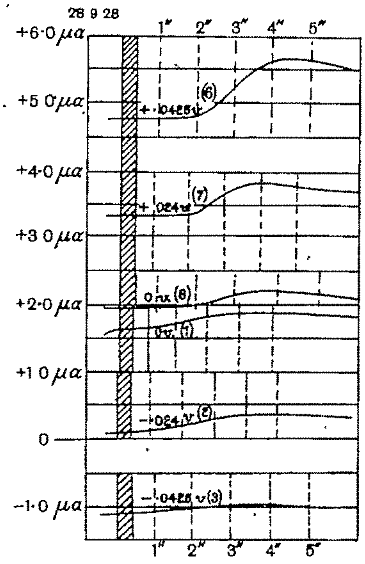
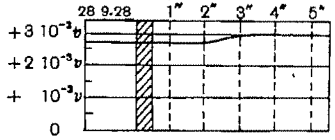
[1] There may, however, be significant variations in curvature which would be apparent to a sufficiently careful examination, or which might be shown by the use of Godefroy's *tachogram* method (4).

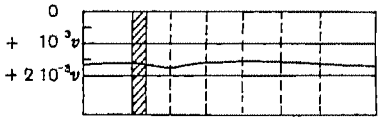
[2] By the principles laid down in Section III, this external resistance would not be large enough to obtain the T-phenomenon undistorted by the P.G.R. if there had been a larger initial deflection. It happened, in this particular experiment, that the initial deflection was very small, so the conditions approximated to those of the Tarchanoff method of neutralizing the initial current.



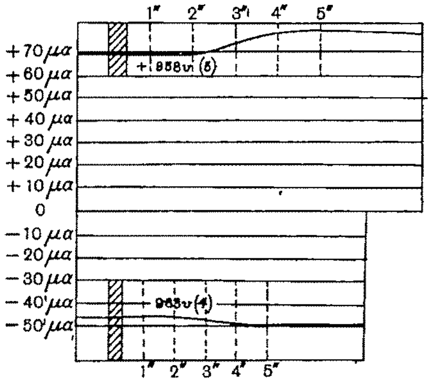
A. Electrical response to stimulation, using small external E.M.F.'s of different strengths (shunt × 10)



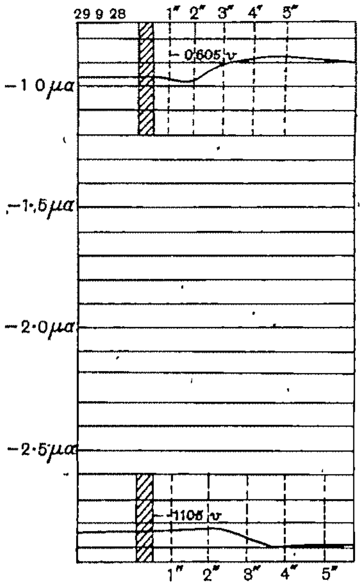
Electrodes as above.



C. T-phenomenon with 200,000 ω external resistance (shunt × 1).



B. As A, but larger external E.M.F.'s (shunt × 300).



D. Part of record like A obtained on a different day, showing reversal of direction of response when external E.M.F. in negative direction is sufficiently increased.

Fig. 2.

change in direction of the deflection in this form of the T-phenomenon, there is a noticeable change of curvature at the point where the negative deflection should appear, which suggests that this reaction is not absent but is simply swamped by the much larger positive change. Weinberg has suggested that the simple form of the deflection is obtained by a subject in a passive condition and the complex form when he is active (6). This is probably part of the explanation of the difference, but I have also obtained both forms of the curve from the same subject in successive observations separated by two or three minutes (for example, in Fig. 2 *C*).

Every investigator of these phenomena has probably at some time considered that both may be exactly the same physical phenomenon measured under different conditions of circuit. A change in potential inside a direct current circuit is indistinguishable from a resistance change. Therefore, the T-phenomenon would produce an apparent change in resistance when we passed a current from an external source through a subject. Conversely, a change in the subject's resistance or polarizability would produce an increase in the current flowing between electrodes on his skin in the conditions we use for measuring the T-phenomenon, even if there were no real change in the skin potential. In other words, the change in apparent resistance in the P.G.R. might merely be the potential change measured in the T-phenomenon, or, alternatively, if the P.G.R. were real, the T-phenomenon might be simply the P.G.R. measured by the E.M.F. of the subject's own skin.

Closer examination shows that these views are untenable and that we are really dealing with two physically distinct phenomena. The evidence for this is:

(1) Each phenomenon can be measured under conditions which make the effect of the other negligible. The methods of doing this will be discussed in the next two sections.

(2) The characteristic difference of form already mentioned. The T-phenomenon frequently shows the three-fold form of Fig. 1 *b*; the P.G.R. never shows this form.

(3) The actual sizes of the two changes is conclusive evidence against their identification. The P.G.R. is commonly a 10 per cent. change of apparent resistance in a 2-volt circuit, while the T-phenomenon is a potential change of less than 10 mv. I have obtained a P.G.R. of 4·5 per cent. in a circuit of 43·5 volts, which if it were due to a potential change would require one more than 200 times as great as the largest T-phenomenon.

Similarly, although the apparent extent of the T-phenomenon must, under many conditions of measurement, be influenced by the P.G.R. phenomenon (for the somatic current itself is influenced by body resistance or polarizability), the T-phenomenon is often far too large to be possibly caused merely by the effect of the P.G.R. phenomenon on the somatic current. Particularly clear evidence of its independence is furnished by the T-phenomenon shown in Fig. 1, for here there is actual reversal of the direction in which the current flows. This could plainly only be caused by a change in potential, for no change in resistance or polarizability could do more than change the amount of the current while leaving its direction unchanged.

The most convincing demonstration of the independence of the two phenomena and of their combined effect on the total current change following stimulation under different conditions of circuit is provided by an experiment in which the current changes to a single stimulus are studied with different small values of external E.M.F. Figs. 2 and 3 show the results of such an experiment.

The stimulus was a loud buzz in a telephone head-receiver from a current interrupted 250 times per second by an electrically driven tuning-fork. A Wundt's time-sense apparatus was used to control the duration of the stimuli which was a little less than $\frac{1}{2}$ sec. The responses were recorded photographically. On the bromide paper was recorded also the period of the stimulus (shown as a shaded area in the photographs) and the time in fifths of a second. The electrodes were Ag/AgCl discs of 21·2 cm.^{2}, placed one on the back, the other on the palm of the subject's left hand[1]. The external current was supplied from an accumulator, connected with a potential divider and a commutator for reversing its direction. The galvanometer was a Moll suitably shunted.

The somatic current was found (as is usual) to flow through the external circuit from the back to the palm of the hand. I have, therefore, adopted the convention of indicating a current or an E.M.F. in this direction as positive, and in the other direction as negative. An external E.M.F. of + 0·0425 volt means, therefore, one in which the anode is on the palm of the hand, the kathode on the back (*i.e.* one which produces a current in the same direction as the somatic current).

Fig. 2 *A* shows the total responses obtained with values of external E.M.F. varying from + 0·0425 volt to − 0·0425 volt. The photographic records have been traced with Indian ink for clear reproduction. The external E.M.F. is written over each curve, while the serial order of the

different observations is shown in brackets. No refinement of the method of administering stimuli can make the response an absolutely constant one, but it will be seen that there is sufficient constancy for the general tendency of the results to be clear. The P.G.R. is proportional to the current strength. The total response is, therefore, largest with 0·0425 volt, where a large P.G.R. is acting in the same direction as the T-phenomenon. As the external E.M.F. is lowered the total response becomes less, and is least at − 0·0425 volt where the P.G.R. and T-phenomenon are acting in opposite directions. The response is, however, still in a positive direction because the P.G.R. response is smaller than the T-phenomenon.

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[1] Details of electrodes will be given in Section IV.

In Fig. 2 *B* are shown the responses obtained in the same series of experiments, with E.M.F.'s of nearly 1 volt strength. These cannot be shown on the same scale as the others because the currents are so much larger. If they were magnified to the same scale, it would be seen that the current changes are also very much larger: + 9·8 micro-amperes for the upper one and $-5{\cdot}6\mu$ amp. for the lower, while the largest deflection in Fig. 2 *A* is $+0{\cdot}87\mu$ amp. The P.G.R. is now very much larger than the T-phenomenon, and (as would be expected) the deflection for the lower curve in Fig. 2 *B* is in a negative direction since the P.G.R. has much more than neutralized the small positive change of the T-phenomenon.

Somewhere, therefore, between − 0·0425 and − 0·963 volt external, the positive deflection of the T-phenomenon has been exactly neutralized by the opposite P.G.R. change. To see more precisely where this change took place, another series of experiments was performed, in which a larger number of small negative E.M.F.'s were used. Two of the results are shown in Fig. 2 *D*. It will be seen that with − 0·0605 volt, there is a positive deflection; the T-phenomenon predominates although reduced in amount by the opposing P.G.R. With − 0·1105 volt, on the other hand, the deflection is negative; the P.G.R. predominates although reduced by the T-phenomenon.

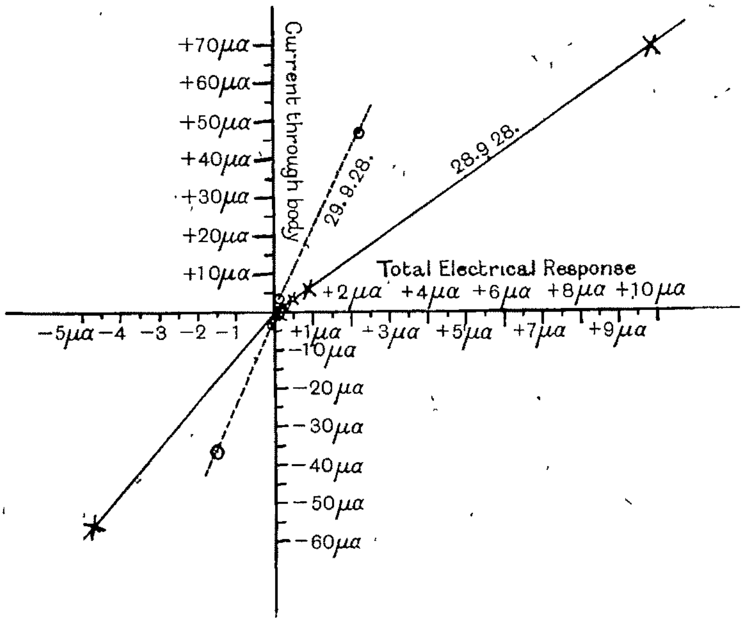
It will be noticed that the whole series of results entirely negatives the statement made by Prideaux that the P.G.R. is absent with small external E.M.F.'s(8). The P.G.R. is present so long as any current flows through the body. Nor is Prideaux's result to be expected on theoretical grounds (on the polarization hypothesis any more than on the resistance hypothesis). A small current produces, it is true, a small back E.M.F. of polarization, but not one which is a smaller fraction of the polarizing E.M.F. The effect on apparent resistance of a change in polarizability will be no less if a small measuring current is used than with a large one. Actually it is the use of a very large current that reduces the P.G.R.

Fig. 2 *C* shows another way of measuring the T-phenomenon in nearly a pure form. A resistance of 200,000 ohms has been put in series with the subject, so that changes in the apparent resistance of the subject (which are only of the order of 2000 ohms) may be rendered of negligible effect in comparison with changes in somatic E.M.F. The initial E.M.F. shown in the upper curve (+ 0·026 volt) is in close agreement with that calculated from the voltage needed in Fig. 2 *A* to reduce the deflection to zero. The T-phenomenon is 0·003 volt, which again is roughly the same as that indicated in Nos. 1 and 8 of Fig. 4 *A*.

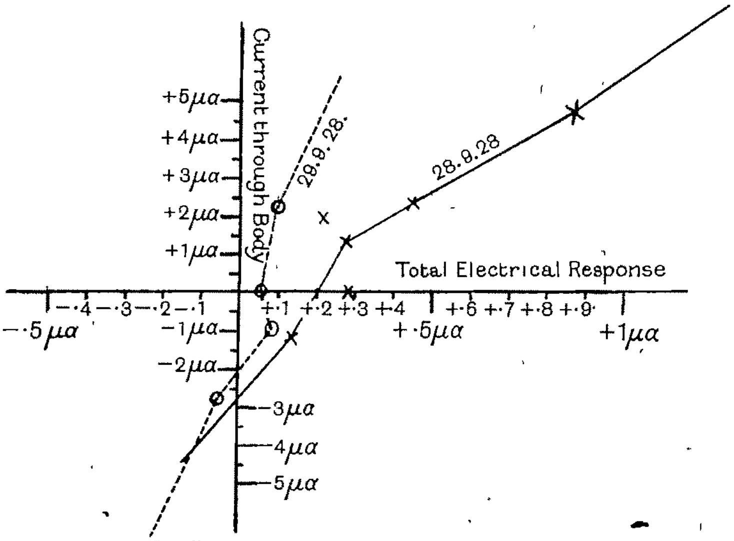
To make sure that the initial current was really a somatic current and did not flow from the electrodes, the position of the electrodes was reversed, the one previously on the back of the hand was placed on the palm, and *vice versa.* The E.M.F. was also found to be reversed, showing that it was mainly from the hand. Its new value was, however, only 0·016 volt. This difference may be due to the electrodes showing a potential difference of 0·005 volt (an unimportant amount), or it may be due to the unavoidable disturbance of the subject. The new T-phenomenon now shows the complex form shown in Fig. 1.

In Fig. 3 these results are shown as a graph in which is plotted the total electrical response to stimulation (*i.e.* the current change due to the P.G.R. and to the T-phenomenon together) at various values of current through the body (*i.e.* current due to the imposed E.M.F. together with the somatic current). Again we have the difficulty that the extreme values, when the external E.M.F. was in the neighbourhood of 1 volt, cannot be shown on a scale which is large enough adequately to represent the smaller values. Fig. 3 *A*, therefore, shows the relation of these extreme values to the others, while Fig. 3 *B* shows the relationship of the values obtained for small external E.M.F.'s on a scale too large to include the extreme values. The points marked by crosses and joined by a continuous line are the values for the responses shown in the photographs of Figs. 2 *A* and 2 *B*, while those shown by circles and joined by a broken line are the complete series of which two members have been shown in Fig. 2 *D*.

If the T-phenomenon were the only electrical response, the curves in Fig. 3 would be straight lines parallel to the base (the change in current would be a constant amount and would not vary with the current strength). If the P.G.R. were the only response, the curves would be straight lines inclined to the axes but passing through the origin (the change in current would be proportional to the current strength). If both are present, the curves will be straight lines inclined to the axes



A. Curve of variation of electrical response with initial current.



B. Central part of A shown on a larger scale.

Fig. 3.

but not passing through the origin. They will cut the horizontal axis at a distance approximately equal to the magnitude of the T-phenomenon alone. While there are inconsistencies between individual readings, which are due to the fact that both phenomena are variable in quantity, it is clear that it is to this last condition that the curves approximate[1]. There is a T-phenomenon which is the main effect when the current through the body is small, while there is also a P.G.R. effect which is approximately proportional to the current strength. While the T-phenomenon may be presumed to remain the same in absolute amount whatever the current strength, its magnitude relative to the P.G.R. effect becomes less as the current strength is increased until at a sufficiently large current strength it becomes negligible. In the next two sections, these results will be used to determine what are the conditions of circuit under which each phenomenon can be measured in a pure form.

II. THE MEASUREMENT OF THE PSYCHO-GALVANIC REFLEX.

For many purposes it makes no difference whether we accept the 'resistance' or the 'polarization' hypothesis. There is, however, a set of practical problems in the work of the experimental psychologist in which the difference between these two hypotheses cannot be altogether ignored. This is the whole range of problems in which we measure the apparent resistance of the subject or its changes, with the intention of comparing magnitudes obtained under one set of conditions with those obtained under another set of conditions. If we wish to make such comparative measurements, we cannot afford to ignore the question of whether we are, in fact, making an absolute measurement of a resistance or whether we are dealing with a mere appearance of resistance.

It must be remembered that we never directly measure resistance in a physical experiment; we measure currents and their changes. Even on a Wheatstone bridge in which we have balanced the subject on the fourth arm of the bridge (Fig. 4 *B*), the quantity $r_2 . r_3/r_1$, which we call his 'resistance,' is in reality a measure of e/i, where e is the E.M.F. applied to the electrodes and i is the resulting current flowing between them. It is much more obvious that we are not directly measuring a resistance if the measure is made in a straight circuit without a bridge (Fig. 4 *A*).

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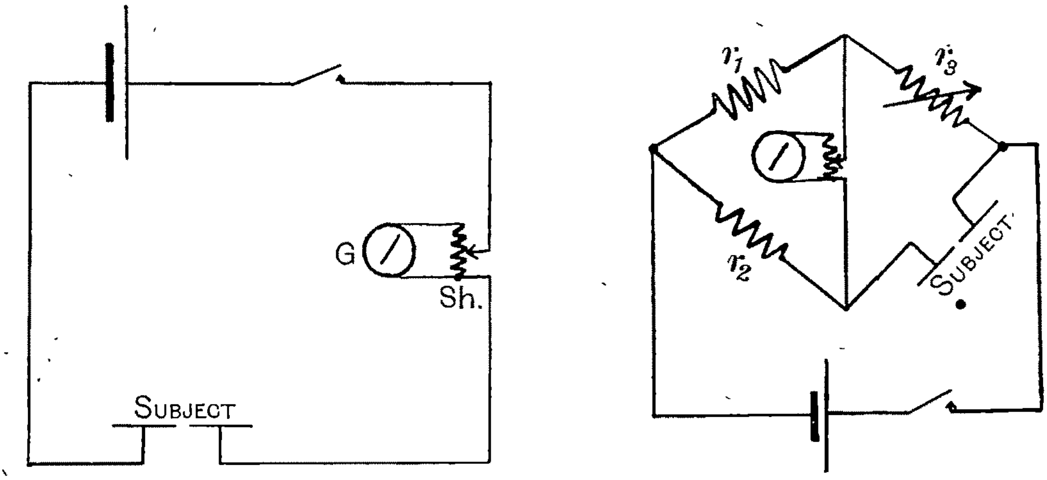
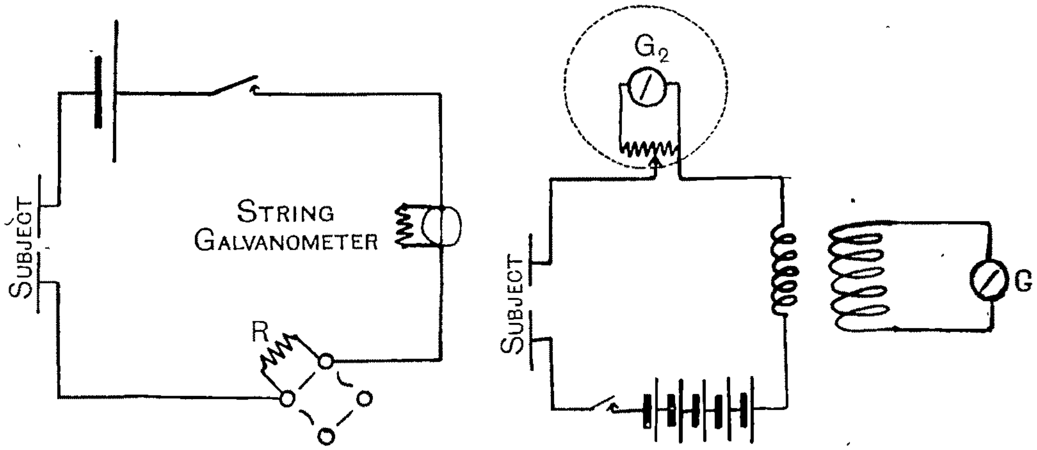
[1] The curve showing the variation of current change with current strength is not exactly a straight line, because with very high external E.M.F.'s (20–40 volts) I find that the P.G.R. measured as percentage change of current becomes measurably less. This I believe to be due to a given change in polarizability affecting actual polarization less as saturation of polarization is approached. This effect is, however, very small and may be neglected when e is 2 volts or less.

If any polarization is present, the current flowing through the subject will be $(e-p)/W$, where p is the back E.M.F. of polarization and W is the true resistance of the subject. The quantity measured as his 'resistance' is in reality e divided by $(e-p)/W$, *i.e.* it is $e.W/(e-p)$. Let us call this quantity the 'apparent resistance' of the subject, and use for it the symbol W'.



A. Straight circuit for P.G.R.

B. Wheatstone bridge circuit.



C. Salomonson's circuit.

D. Godefroy's circuit.

Fig. 4.

If there were no polarization (so e/i was a direct measure of W), we could be content to measure e/i and state simply that using electrodes of a certain size on a certain part of the subject's body, we had found that his resistance was, let us say, 11,000 ohms. This quantity would be an absolute measurement of a simple physical fact. But on no theory (neither Gildemeister's nor Einthoven's) is this the case. W' is a quantity which will be different when measured in different ways. We cannot be content to say that the apparent resistance is 11,000 ohms because the fraction $e.W/(e-p)$ will have a value dependent on the strength of current used in measuring it. With larger E.M.F.'s, p becomes a smaller fraction of e, and the apparent resistance is thus decreased. This means that a mere statement of W' is always insufficient. We must also state the value of e, or of i, or of both E (the E.M.F. of the battery used) and also the total external resistance in the circuit.

One of the most serious consequences of the realization that W' is not a resistance and that $\Delta W'$ (the change in apparent resistance) is not a resistance change is the consequent uncertainty as to what is a significant numerical measure of the extent of the reaction for different values of W'. Let us suppose that we are doing a word-reaction experiment. We call out the word 'dog' and our subject's apparent resistance changes from 10,000 ohms to 9000 ohms. Ten minutes later when we come to the word 'cat,' his apparent resistance is 7000 ohms. What change of resistance would indicate that his reaction to 'cat' was equal to that to 'dog'? Commonly it is assumed that such equality would be shown by a change of 700 ohms (*i.e.* by an equal percentage drop in apparent resistance), but for no sufficient reason. If $\Delta W'$ were a resistance change, it would be a reasonably certain assumption that $\Delta W'/W'$ would be constant for equal reactions, but if the change is really one of polarization and $\Delta W'$ is a somewhat complicated function of the change of polarization, it ceases to be certain that $\Delta W'/W'$ is the significant measure of the P.G.R. On the polarization hypothesis, this is a serious difficulty, gravely limiting the probability of comparisons between the extent of the P.G.R. at different apparent resistances being significant. The problem of what is a significant measure of the P.G.R. is not yet solved. The question will be further discussed in a later section.

It remains true that we must express our results somehow. It is practically convenient to use e/i as a measure and to call it the 'apparent resistance,' and to express the P.G.R. as $100 . \Delta W'/W'$, and to call this 'the percentage change in apparent resistance.' There is no harm in this so long as it is realized that it is no more than a practically convenient device with no precise physical meaning.

A further complication in the measurement of W' is indicated by the curves shown in Fig. 2. When measuring the resistance of our subject with an external E.M.F., the potential difference between the electrodes is not simply that imposed by the external battery. There is also an E.M.F. of somatic origin whose changes are measured in the T-phenomenon. This E.M.F. we may call ϵ. The current through the subject will be $\{e + \epsilon - p_{(e+\epsilon)}\}/W$, where $p_{(e+\epsilon)}$ is the back E.M.F. of polarization produced by the combined action of e and ϵ.

We must either measure ϵ and make the necessary correction in our formula for calculating W', or else make e so large that ϵ is negligible in comparison. If, for any reason, we want to measure W' with small values of e the former course must be adopted. It is better to avoid it, however, if possible, since ϵ is a highly variable quantity so such a correction produces an element of unreliability in the results obtained. For most practical purposes, it is better to make e so large that ϵ may be ignored.

To get some idea of how large e should be for this purpose, we may calculate from the results given in Fig. 3. These, in fact, are very suitable for the purpose since the value of ϵ is unusually large.

The true value of W' will be $(e + \epsilon)/i$; if, however, we ignore ϵ, we shall take W' as e/i. The error involved is therefore measured by ϵ/e. In Fig. 2, ϵ was 0·025 volt. The error in the measurement of W' with an external E.M.F. of n volts would therefore be $2{\cdot}5/n$ per cent. For No. 5 in which nearly 1 volt external was used, the uncorrected value of W' is 13,900 ohms; corrected for ϵ, it is 14,300 ohms. For No. 4, W' is 20,200 ohms uncorrected, 19,700 ohms corrected[1]. It is very rarely in practice that we want to determine W' with such precision that an error of 2 or 3 per cent. is of any importance. So far as ϵ is concerned, we can be content with any value of e greater than 1 volt. If we use a much smaller voltage than this, we must be prepared for a correspondingly large (and variable) error. A more important consideration limiting the smallness of e is the necessity of seeing that P.G.R. measurements are not appreciably affected by changes in ϵ, *i.e.* that the measurement of the P.G.R. is not affected by the influence of the T-phenomenon. This also is secured by making e sufficiently large, since the change in current of the P.G.R. is roughly proportional to the absolute current through the body (*i.e.* roughly to e), while the current change of the T-phenomenon is independent of e.

Again, we can turn to the measurements shown in Fig. 2 as an indication of how great will be the influence of the T-phenomenon on the P.G.R. at different values of e. The top record of Fig. 2 *D* shows a T-phenomenon of about 3 mv., that indicated by No. 2 of Fig. 2 *A* is between 3·5 and 4 mv. No. 4 of Fig. 2 *B* shows a P.G.R. of 10·55 per cent., No. 5 of 11·55 per cent.

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[1] It will be noticed that, even after this correction, a very large difference remains between the two values of W' obtained by external currents in opposite directions. This is a real difference which is always found, and has nothing to do with the error in measurement produced by ϵ.

Let us consider the effect of a T-phenomenon of 4 mv. on a P.G.R. of 11 per cent. To a very close approximation, a change in somatic E.M.F. of $\Delta\epsilon$ will simulate a change in apparent resistance of $100 . \Delta\epsilon/e$ per cent. The measured P.G.R. will be increased by that amount if the external current is passed through the hand in a positive direction, and correspondingly diminished if the external current is in a negative direction. We can reasonably be content with an elimination of the effects of $\Delta\epsilon$ which makes these effects not more than a twentieth of the P.G.R. With an external E.M.F. of 1 volt, the fraction $100 . \Delta\epsilon/e$ becomes 0·4 per cent. Since the measured P.G.R. is about 10 per cent., this is less than one-twentieth of the P.G.R., and the T-phenomenon may be regarded as satisfactorily eliminated if the measurement of the P.G.R. is made with an external E.M.F. of 1 volt. A reasonable margin of safety is given if we double this value and measure the P.G.R. with an e of 2 volts[1].

These figures have, of course, no universal validity, being derived from one set of measurements. They may be regarded as a rough but practical indication of the conditions of safety of P.G.R. circuits with electrodes on the back and palm of the hand. With electrodes in other positions, or for any P.G.R. measurements of high precision, the influence of ϵ and of $\Delta\epsilon$ should be determined by measurement and calculation.

The opposite problem of how the T-phenomenon may be measured with satisfactory elimination of the effect of the P.G.R. will be discussed in the next section.

A final point which must be considered here is the practical effect of the dependence of both W' and $\Delta W'$ on e. On no hypothesis is W' an absolute measurement; on Gildemeister's hypothesis, $\Delta W'/W'$ also is not. We shall be safe in saying that both measurements are functions of the measuring E.M.F. If any comparative use of P.G.R. results is to be made, the control of e is imperative.

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[1] This refers, of course, to the actual potential difference between the electrodes, not to the E.M.F. of the battery used. If large external resistances are introduced into the circuit, e will be considerably smaller than E, and with a badly arranged circuit a battery of 2 or more volts may give no guarantee that e is large enough to eliminate the T-phenomenon. In a straight circuit, $e = E . W'/(R + W')$, where R is the external resistance. In a balanced Wheatstone bridge with negligible external resistance, $e = E . W'/(W' + r_1)$.

An idea of the magnitude of the effect of increasing external E.M.F. on W' and on $\Delta W'$ can be obtained from the following table which shows mean values of W' and $\Delta W'$ obtained in successive applications of the same sound stimulus (exactly controlled in intensity and duration) to a subject with Ag/AgCl electrodes of 21·2 cm.^{2} on the back and palm of the right hand (anode on the back).

Table I. *Variation of apparent resistance and* P.G.R. *with external* E.M.F.[1]

e (volts)	W'	$\Delta W'$ (as % of W')
2·08	7630	18·6
22·8	4640	6·4
33 1	3010	3·15

It is probable that the variations of P.G.R. with an e of less than 2 volts are much smaller than those shown in the above table. I have not found it possible to establish significant differences for these lower values, because (*a*) the differences (if present) are small, (*b*) the P.G.R. appears to be more variable with low values of e, and (*c*) the reliability of their measurement is seriously disturbed by the simultaneous presence of the T-phenomenon. It would, however, be rash to assume that variations of $\Delta W'/W'$ with e are absent for low values of e, and we shall be on the side of caution if we treat e as one of the factors that must always be controlled in order to get significant values of W' and of $\Delta W'$.

This leads to a further practical consideration. W' is a measure whose numerical value depends on the E.M.F. used to measure it. Alternatively we can say that it depends on the strength of current i used to measure it. After a P.G.R. has taken place, W' may have altered so much that there will be a considerable difference in i, and in some conditions of circuit in both e and i. We may therefore be measuring W' under different conditions before and after the reaction, so that our measure of W' is partly a product of this difference in conditions.

Both practical and theoretical considerations suggest that the resultant error is not a large one. It can, however, be so easily eliminated altogether that it is worth while to arrange the circuit for this end. What is required is that either i or e should be kept constant (*i.e.* independent of W'). Theoretically, i could be kept constant by having an external resistance in series with the subject so large that $\Delta W'$ was negligible in comparison with it. The E.M.F. from the battery would also be made correspondingly large, and the P.G.R. would be measured by a high resistance galvanometer in parallel with the subject. In practice, this would be a very inconvenient arrangement. The alternative, to keep e constant, is secured very simply by using a straight circuit in which the total external resistance is negligible in comparison with W'. This can easily be arranged by suitably shunting the galvanometer and by keeping all other resistances out of the circuit. A considerable external resistance in a straight circuit is, of course, also objectionable because it reduces the sensitivity of the measurement of $\Delta W'$. The commonly used Wheatstone bridge circuit never fulfils the condition that e should remain the same after a change in W'. This is one objection to the use of the Wheatstone bridge circuit for P.G.R. measurements. Other objections to it will be mentioned later.

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[1] These results are only given as indication of the general results of increase of e. They are means from an insufficient number of observations to be regarded as highly reliable. The general indications of these results were confirmed by a reliable series taken with different electrodes on another part of the hand. These have not yet been published.

After long experimentation with alternative ways of measuring the P.G.R., I have come to the conclusion that the obvious and straightforward circuit shown in Fig. 4 *A* is the best. This includes simply a battery of 2 or more volts, the subject, and a shunted galvanometer. There will be an initial deflection of the galvanometer which will be a measure of the reciprocal of the subject's initial apparent resistance. Changes in this deflection after stimulation give a measure of the change of conductivity due to the P.G.R. The only objection to this method is that the deflections obtained are much smaller than those of the Wheatstone bridge method. This would be a serious practical objection if the P.G.R. were a small change. In fact, however, it is generally of the order of about 10 per cent. and a change of this size can be measured with sufficient accuracy in a direct circuit. For most purposes, there is no advantage in over-refining reliability of measurement since the phenomenon itself shows uncontrollable variations which are a considerable proportion of the whole reaction. We may, without difficulty, make our initial deflection as much as 40 cm.; the P.G.R. change of deflection will then be about 40 mm. and may be read with an accuracy of 0·5 mm. (*i.e.* with an error of the order of 2 per cent.). This assumes that the phenomenon is measured visually. In photographic recording, the deflections must be smaller, but also the accuracy of reading is greater (I have found, in practice, that the error does not exceed 0·2 mm. with good records). With a P.G.R. change in deflection of 10 mm. (which can easily be secured by suitable shunting) this gives an accuracy of the same order.

If a higher accuracy than this is required, or if the P.G.R.'s measured are much smaller than 10 per cent., the investigator is driven to use the Wheatstone bridge method (Fig. 4 *B*) in spite of its many disadvantages.

It is much to be regretted, however, that this is commonly regarded as the standard method of measuring the phenomenon and is taught to students as such.

The physicist uses the Wheatstone bridge for the delicate measurement of an unknown resistance X, which is placed in the fourth arm of the bridge (occupied in the diagram by the subject). An external battery is used and r_3 is adjusted until no current flows through the galvanometer. The value of X is then discovered from the relationship $X/r_3 = r_2/r_1$. This is called a 'null' method of measurement, because no current is flowing through the galvanometer at the point of balance. Since the current will be infinitesimal in the last stages of balance, the galvanometer may be made very sensitive; hence the high precision of this method of measuring resistance.

In making P.G.R. measurements, we place our subject in the position of X, and measure his apparent resistance as $r_2 . r_3/r_1$. It is true that this is not his ohmic resistance but is compounded of his true resistance and a back E.M.F. of polarization. This, however, introduces no new complication here for the W' measured on the Wheatstone bridge is the same as that which would be measured in a direct circuit by the same current.

Serious difficulties arise, however, from the fact that we cannot use a null method to measure a changing resistance. The bridge cannot be balanced both at the beginning and end of the reaction. At best, we can have it exactly balanced at the beginning (although this is often difficult). The deflection of the galvanometer in an unbalanced bridge bears no simple relationship to the change of resistance in the fourth arm, and is also a function of the value of r_3. What then are we to take as a measure of the P.G.R.? To collect varying values of the deflection obtained on an unbalanced bridge even with r_3 constant (more obviously if it is changed during the experiment) and to call these values of the P.G.R. phenomenon, is clearly fallacious. To take the average of such a collection of physically meaningless figures and to call it the mean P.G.R. is worse.

Two courses remain open to the experimenter. He can calculate the value of W' which would give each observed deflection, or he can determine it empirically. The simplest formula for calculation (that in which there is only negligible resistance external to the bridge circuit) is as follows: i (the current through the galvanometer)

$$= \frac{E\,(r_1 W' - r_2 r_3)}{g\,(r_1 + r_3)\,(r_2 + W') + r_1 r_3\,(r_2 + W') + r_2 W'\,(r_1 + r_3)}$$

(where g is the resistance of the galvanometer).

In practice, the best method is, however, to replace the subject by a variable resistance and to measure separately for every P.G.R. obtained the value of this variable resistance which will give the same initial and final deflections in the same conditions of circuit. This is laborious, but less so than calculation from the formula. This is the method which should always be employed when the Wheatstone bridge is used for P.G.R. measurements. It is almost unbelievable that serious investigators have evaluated their results by placing a small constant resistance in series with the subject, have discovered the change in deflection when this is short-circuited with the subject still in the circuit, and have calculated $\Delta W'$ on the assumption that it is a simple function of the change in deflection. Figures obtained by such methods are entirely worthless.

There is a last practical objection to the Wheatstone bridge circuit, which is due to the fact that sudden and unexpected changes in the subject's resistance may occur during the course of an experiment. These may easily drive the galvanometer off the scale or even damage it seriously. A change in resistance which would double the current through the galvanometer on a direct circuit may make it a hundred times as great on a Wheatstone bridge circuit. This limits the central advantage of the Wheatstone bridge circuit (that the galvanometer may be made highly sensitive) just when we need it most: with a string galvanometer, for example, which will not give us very precise values in a direct circuit because we cannot pass a sufficiently large initial current through it to give an initial deflection of satisfactory size.

The case against the Wheatstone bridge circuit is heavy both on theoretical and practical grounds. It remains true that the investigator must sometimes use it, and face the labour of calculation or calibration. There is no case for teaching it to students as the standard method. Its theory and practice are complicated. These complications can only be ignored by using inaccurate methods. The straight circuit, on the other hand, is easy to use and is intelligible to the student who has little knowledge of physics.

Fig. 4 *C* shows a circuit used by Prof. Wertheim Salomonson, with the intention of measuring simultaneously the polarization and the true resistance of the subject (9). An external E.M.F. was used and an external resistance R was introduced and short-circuited in rapid alternation by means of a mechanically driven commutator. The currents i_1 and i_2 in these two conditions of the circuit were measured by means of a string galvanometer. W and p were then calculated from the formulae:

$$i_1 = (E - p)/(W + R), \text{ and } i_2 = (E - p)/W.$$

This method assumes that during the short period of each change of the commutator, p will be unchanged. Gildemeister's work shows, however, that this is not the case. A very considerable effect of p on W' is found with alternating currents even of a period of several thousand alternations per second. In fact, what is measured by Salomonson's method is not W (the true resistance) but simply a value of W' which is less in amount than the W' measured with a direct current to an extent dependent on the speed of the commutator. The method fails in its purpose of measuring W, and does no more than could be done equally well and more conveniently by simply using an alternating current for the measurement of W'.

I have also used a modification of Salomonson's method for simultaneously measuring the P.G.R. and the T-phenomenon. For this purpose, the battery is omitted from Fig. 4 *C*, and the string galvanometer is made sufficiently sensitive to give good deflections with the somatic current alone. Then $i_1 = \epsilon/(W' + R)$, and $i_2 = \epsilon/W'$. From these formulae the values of ϵ and W' can be calculated along the whole length of the curve. It was found that the method was practicable, although it was laborious and inaccurate, the main source of inaccuracy being the unreliability of the string galvanometer at such high sensitivity. Simultaneous measurement of the two phenomena could probably be better carried out by means of two sets of electrodes on different parts of the skin.

Fig. 4 *D* shows the circuit recommended by Dr Godefroy (4). A current from an external source is passed through the subject and through the primary of an induction coil. The secondary of the induction coil is connected in series with a sensitive galvanometer G_1. So long as the current in the primary circuit is constant, no current flows through G_1, but an increase of current in the primary circuit causes an induced current to flow through G_1 in one direction, while a decrease produces an induced current through G_1 in the opposite direction. What is measured by G_1 is thus not the current through the subject but the changes in that current. If we can ignore the effect of self-induction in the secondary circuit and the inertia of the galvanometer mirror, the deflection of G_1 is proportional to di/dt. In order to get sufficiently large deflections of G_1, it has generally been found necessary to use an external battery of about 10 volts. In order to record at the same time the absolute value of the current in the primary circuit, a second galvanometer G_2 is sometimes inserted in that circuit. Without this second galvanometer there is no indication of the value of W'.

The advantages claimed for this method are (1) that the galvanometer deflections vary about zero so that there are no adjustments to make during the course of an experiment to keep its movements within convenient limits, and (2) that the curve it gives (the *tachogram*) is of far more distinctive form than the simple P.G.R. curve, and so gives more hope of solving the important practical questions of objective differentiation between different emotional reactions.

I have no practical experience of the use of this circuit, so the following comments are of a purely theoretical order.

(1) The tachogram curve contains nothing that is not present in the P.G.R. curve. Every deflection in the tachogram is present as a curvature change in the P.G.R. If a P.G.R. were obtained with a galvanometer whose deflections were proportional to the current and whose period was negligible, and the inclination of the curve at different times was plotted on a graph, the tachogram would be the curve obtained. On the other hand, changes in curvature are not easily seen, and the practical advantage of Godefroy's method of transforming them into deflections, if they are to be studied, is very great.

(2) While all information contained in the tachogram curve is implicit in the P.G.R. curve, the converse is not true. The tachogram tells us the shape of the P.G.R. curve but not its level; it contains no indication of the absolute value of W'.[1] In an earlier investigation, I tried to show that the absolute value of W' has a certain significance of its own, being partly dependent on the degree of 'alertness' of the subject (10). Since we are not wise in an investigation to cut ourselves off from any source of information about what we are investigating, this consideration alone suggests that the galvanometer G_2 should never be omitted.

(3) There is, however, a more serious reason why a method of measuring W' cannot properly be omitted from this circuit. We have seen that D (the deflection of G_1) $\propto di/dt$. We may usefully carry the calculation further in order to discover how D is dependent on the changes in W'.

$$i = e/W', \text{ and } D = k.di/dt,$$

therefore $$dW'/dt = \frac{dW'}{di}\cdot\frac{di}{dt} = -\frac{W'^2}{e}\cdot\frac{D}{k},$$

or $$D = -k\frac{e}{W'^2}\cdot\frac{dW'}{dt}.$$

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[1] Because there is no unique solution to the differential equation $x = \int \frac{di}{dt}.dt$.

The tachogram deflection is thus not proportional to the rate of change in W' but to the ratio of this rate to the square of W'. This brings us to a variant of a problem already discussed, that of a significant numerical measure for the P.G.R. If two tachogram deflections obtained at different times with the same e are equal, we are naturally inclined to say that these two reactions (perhaps to two different stimulus words) were equal. But what it means in physical fact is that the maximum value of dW'/W'^2 was the same in each case. W' may have been different; so if some other function of W' had been measured (let us say dW'/W'), the two reactions would have appeared unequal. Clearly we have no reason for supposing that dW'/W'^2 rather than any other function of W' is equal for equal emotional reactions. If we know W', this is a no more serious difficulty with the tachogram method than with the ordinary P.G.R. method. It means simply that we are not sure of any method of making significant comparisons between the sizes of different deflections, unless these have been obtained with equal values of W'. It becomes a fatal objection to any numerical comparison of tachogram figures obtained in a circuit in which G_2 has been omitted so long as W' is unknown. In such a circuit, there is no indication of how W' has varied during the course of the experiment, so the numerical record of the sizes of the deflections is simply meaningless.

There are other possible circuits for the measurement of the P.G.R., as, for example, the Kohlrausch bridge circuit with alternating current used by Gildemeister (1). Such circuits are, however, only useful for special purposes, and are not required in the ordinary course of investigation.

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SUMMATION AND SUBTRACTION OF BRIGHTNESS IN BINOCULAR PERCEPTION

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I. Statement of problem.

Since homonymous halves of both retinae are connected with the same half of the cerebrum, it is usually assumed that corresponding points upon the two retinae outside of the fovea are represented in the visual projection areas of the cortex by single patterns. It is therefore generally accepted that the two retinae function integratively, *i.e.* that the singleness of vision with binocular observation is made possible by the unity of their central connections. Numerous results gathered from observations of stereoscopic phenomena such as retinal rivalry support the view that alteration of conditions upon one retina always exerts some influence on the functioning of the other retina. Consequently it is rather surprising that Fechner[1], Sherrington[2], Abney and Watson[3], Dawson[4] and others have claimed to have proved by experiment that there is no binocular summation of brightness, or, in other words, that though the eyes function integratively for other conditions they have little appreciable antagonistic or facilitative influence upon one another as far as brightness is concerned. To be sure, other investigators, such as

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[1] See v. Helmholtz, *Handbuch d. Physiol. Optik*, 3rd ed. II, 287.

[2] C. S. Sherrington, *Integrative Action of the Nervous System*, pp. 354–93.

[3] Abney and Watson, *Phil. Trans.* A, 1916, CCXVI, 109.

[4] Dawson, this *Journal*, 1917, VIII, 510.

McDougall,[1] and some earlier men, like Valerius[2], Piper[3] and Aubert[4], claimed to have found a slight summation effect. However, as the weight of argument from the standpoint of numbers of investigations, prestige of investigators and use of refined apparatus is decidedly against it, binocular integration as regards brightness has come to be a generally discredited fact in the literature[5].

It was the purpose of this investigation to attempt to put this problem of binocular summation of brightness to a critical test, and in particular to determine if possible why the experimental results should be so flatly contradictory. As Sherrington's investigation was the most elaborate one and represents a direct attack upon the problem, we will begin by referring to certain important items in his results.

II. UNEXPLAINED RESULTS FROM SHERRINGTON'S INVESTIGATION.

A careful perusal of Sherrington's article in which he claimed to have refuted the notion that there is binocular summation of brightness will reveal a number of candid statements in which he admits that small amounts of summation were observable. For example, on p. 364[6] we read: "The binocular arrangement, then, is said by them to require a slightly higher frequency for extension of flicker than does the uniocular.... (p. 365). From these observations it appears that similar phases of flickering illumination, if timed to fall coincidently on conjugate retinal areas, do *very slightly reinforce* each other in sensation, and, if timed exactly alternately, do *very slightly mutually reduce*. But the *broad outcome of the observations is that,* so far from bright phases at one eye effacing dark phases at the corresponding spot of the other eye, *there is hardly a trace of any such interference*." On p. 372 we find this: "In certain instances the binocular combination did appear just distinctly the brighter....But there occurred frequent instances in which no excess was observed in the brightness of binocular combinations over that of their carefully balanced uniocular components."

We do not of course reproduce these excerpts in order to cast any suspicion upon Sherrington's conclusions. His conclusions follow logically from his results. In the next section, however, an attempt will be made to sum up the reasons why Sherrington obtained no conclusive results favouring summation and why we did obtain conclusive results.

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[1] W. McDougall, *Mind*, N.S. 1901, x.

[2] Valerius, *Poggendorff's Annal.* CL, 17.

[3] Piper, *Stach. f. Psychol. u. Physiol. d. Sinnesorg.* 1904, XXXII, 161.

[4] Aubert, *Physiol. d. Netzhaut*, Breslau, p. 287.

[5] J. H. Parsons, *Colour Vision*, pp. 57, 142.

[6] *Op. cit.*

III. PRECAUTIONS NECESSARY IN AN INVESTIGATION OF THIS NATURE.

1. In the first place we should like to point out that an individual is a very poor judge of relative brightnesses[1]. On account of the handicap which subjects suffer in judging brightnesses under ordinary circumstances, we concluded that the ideal apparatus to use for this problem ought to possess two comparison surfaces, one of which might be varied uniocularly and the other binocularly. That is to say, we wished

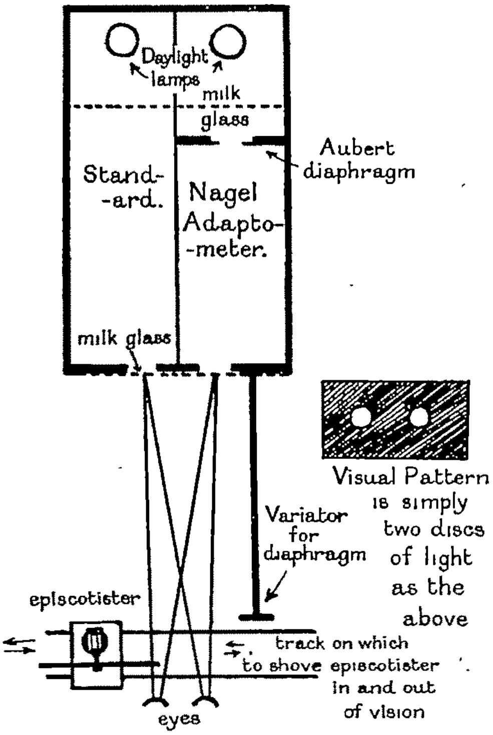

Fig. 1

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[1] In *Photometry*, Van Nostrand, 1926, J. W. T. Walsh emphasizes the neglected fact that "The enormous range of sensitivity possessed by the eye can readily be appreciated from the fact that the ratio of the brightness of objects seen by direct sunlight to that of the same objects seen by starlight on a clear moonless night is at least 10 million to 1.... This power of adapting itself to different conditions of lighting makes the eye useless for the direct measurement of luminous energy, and, as has been said already, photometry depends on the eye solely for the determination of the equality of two adjacent fields as regards either brightness or contrast." The 'slight' subjective differences of brightness such as those reported by Sherrington's subjects might have been found to be of considerable value in spite of the observers' evaluations of 'very slight,' if the experimental conditions had been such that they could have been measured.

to arrange a situation in which the light coming from one field to one eye could be varied and measured and that at the same time light from a standard field which always reached both eyes could be altered. In this way, the observer would always have a standard present by which to make his judgment. We constructed the apparatus represented in Fig. 1 to satisfy these conditions.

2. Statistical methods were introduced in order to overcome errors produced by adaptation, induction and other undetermined variables. Sherrington discovered that 'in certain instances' binocular combinations appeared brighter and that in other instances which he concluded were more frequent, the binocular combinations did not appear so bright. We wished to obtain a measure of the frequency of this predominance so as to be able to say something definite regarding the average instance. Accordingly, we made hundreds of observations and averaged the results.

3. We foresaw that summative effects might be more pronounced with different ratios of uniocular and binocular illuminations and so, after a preliminary experiment to determine the most critical positions, we adopted the following nine ratios of reduction of light: 1/16, 1/8, 1/4, 1/2, 3/4, 15/16, 47/48 and total[1].

4. We decided to perform our first experiment in the dark in order to offset simultaneous contrast influence of background upon the figure[2]. Adoption of dark surroundings also aided in obscuring the presence of the episcotister blade which sometimes causes a displacement of the observer's depth fixation and produces a filmy shadow effect. Another advantage of using dark surroundings is that it reduced variations in pupillary sizes to a minimum. Under the circumstances, artificial pupils were unnecessary.

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[1] As the Talbot Law was at one time supposed not to work with low intensities, we tested its accuracy under our experimental conditions by comparing our results with those obtained by cutting down the illumination with Bausch and Lomb neutral tint filters with ratios of 1/2, 3/4, and 7/8. We found that the dimming effect was practically the same with the filters as with the episcotister. Hyde, working at the Bureau of Standards, found that the law applies accurately over a wide range of physical intensities. Cf. Hyde, *Bull. of Bureau of Standards, Washington, D.C.* II, 1, 1906.

[2] We are working on the summative and subtractive effects with lights of higher intensities at the present time. Binocular summation does seem to hold equally well here as with low illuminations.

IV. PROCEDURE.

The nature of our procedure of investigation was as follows: The observer first attempted to equate the brightness of the two lighted fields by turning a handle to vary the brightness of the right-hand field which was controlled by the adaptometer. After making an equation, an episcotister disc rotating at a velocity well above the critical frequency for fusion was brought in from the left just far enough so that it came between the observer's left eye and the left-hand illuminated field. The observer's view of the right-hand field through his left eye was always unimpeded[1].

Episcotister discs of the different reduction ratios listed in the first column of Table I were employed in irregular order. After the left-hand object had been dimmed somewhat by interposing the episcotister blade, the observer was asked to equate the two fields by altering the right-hand field which was seen by both eyes. Invariably it was found that he diminished the right-hand field in order to equate it to the left-hand field which was dimmed upon one eye only. The proportion of diminution of the left-hand field by the episcotister was measured and the results are diagrammatically represented in Fig. 2. After 25 observations were made, the observer changed places with the experimenter. When two periods of 25 observations were completed, a rest interval was introduced and the subjects left the room and became daylight adapted. One hundred double equations were gathered for each reduction ratio listed in the first column of Table I. Consequently the data for the five series depicted in Table I represents 8400 separate observations for each observer.

The advantage of this procedure over other procedures for studying binocular integration is unquestionable. Many of the earlier observations were made by the method of successive comparison of brightness, which, on account of the delay between presentations to be judged, is extremely difficult, and is never used in photometric work on account of its crudeness. Other investigators have employed a method of simultaneous comparison with a stereoscope, where the observer was asked to report upon the combined effect of two identical superimposed fields which differed only in brightness. This method of studying binocular effects is subject to considerable inaccuracy because, unless a standard is common to both eyes, there is no accurate way by which to make a simultaneous discrimination between the combination of two dissimilar uniocular effects and the binocular integration of two similar effects, except upon the basis of memory. Our apparatus and procedure allows such a binocular standard for comparison and represents a considerable advance over the successive procedure method or the stereoscope procedure

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[1] A study is being made at the present time of left-eye and right-eye differences.

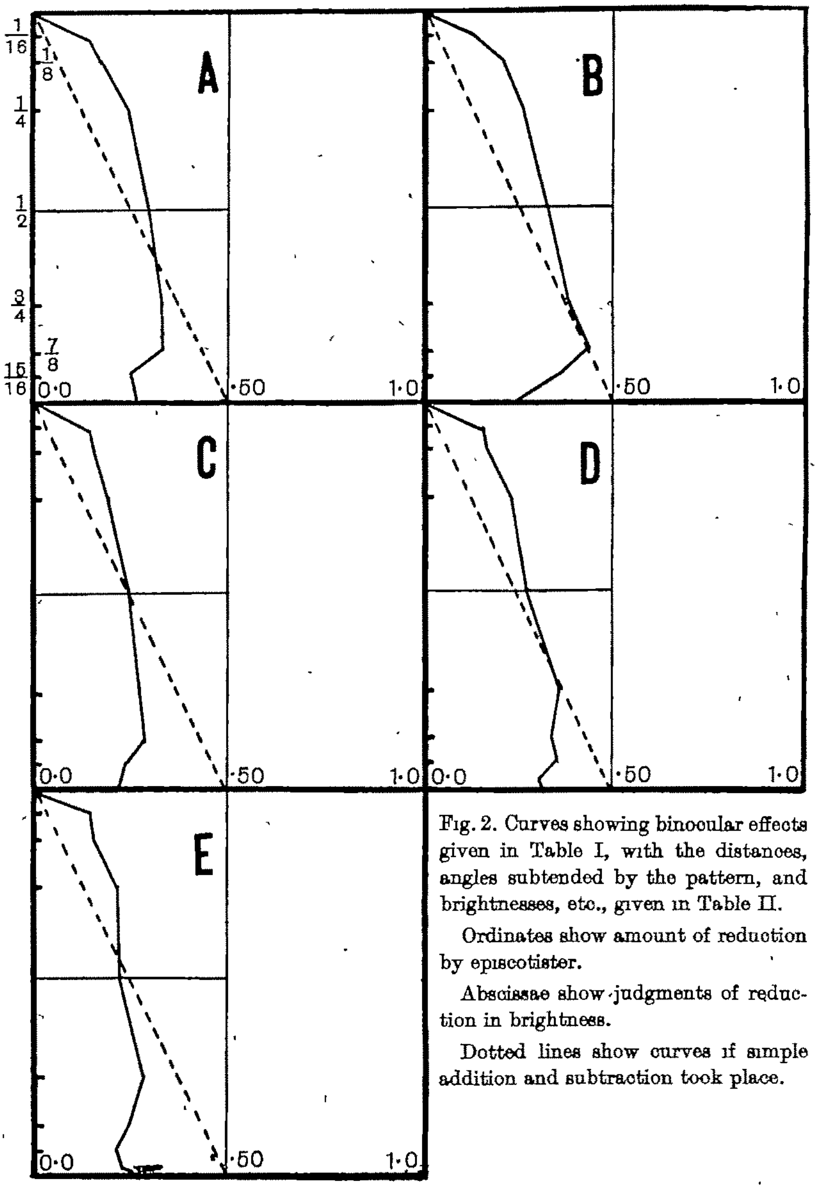

Fig. 2. Curves showing binocular effects given in Table I, with the distances, angles subtended by the pattern, and brightnesses, etc., given in Table II.

Ordinates show amount of reduction by episcotister.

Abscissae show judgments of reduction in brightness.

Dotted lines show curves if simple addition and subtraction took place.

method which is ordinarily a successive procedure method also, since it depends upon memory.

Casual observations of the subjects bear out this emphasis upon the importance of our procedure. We found that when the light coming from one field to one eye was dimmed that there was a difference in brightness between the two fields, but we were not at all certain as to how much difference there was. In fact, sometimes we were not positive that there was any difference. However, as soon as we began to change the illumination of the field which was undimmed to both eyes we became subjectively much more certain that there was a difference in brightness between the two fields and that we could adjust for this discrepancy. It was found that our equations for the brightness of the two fields when both were undimmed varied up and down from observation to observation, but that the adjustments made when one was dimmed by the episcotister also varied up and down in parallel fashion. Thus, by making an equation afresh each time, we cancelled out the uncontrolled general level fluctuations in sensitivity of the two eyes.

Table I.

Reduction	A	B	C	D	E
1/16	0·174	0·172	0·148	0·156	0·140
1/8	0·188	0·208	0·162	0·166	0·165
1/4	0·243	0·257	0·190	0·229	0·213
1/2	0·293	0·304	0·248	0·282	0·229
3/4	0·317	0·372	0·279	0·359	0·280
7/8	0·317	0·428	0·282	0·339	0·265
15/16	0·251	0·366	0·239	0·354	0·211
47/48	—	—	—	0·300	0·239
Total	0·276	0·258	0·217	0·305	0·258

Table II.

Case	Distance cm.	Angles subtended		Disc area	Flux lumens	Brightness millamberts	Retinal illumination in photons
		Disc	Pattern				
A	147	2′ 5″	10° 56′	31·2	0·00285	0·091	2·10
B	147	2′ 5″	10° 56′	31·2	0·00570	0·182	4·06
C	147	2′ 5″	10° 56′	31·2	0·01425	0·455	9·78
D	71	5′ 6″	23° 00′	31·2	0·01425	0·455	9·78
E	420	51′ 4″	2° 33′	31·2	0·01425	0·455	9·78

We checked the results of our own observations at various points with the results of casual observers. We found that the results from two trained observers in psychology and the results from a trained astronomer, who was accustomed to judge brightness of stars, correlated closely with our own results.

V. RESULTS.

Fig. 2 represents graphically the composite results of two observers who are the authors of this paper. As their results varied only in minor details, we are representing the average for the two observers in the form of composite curves.

The curves *A*, *B*, *C* summarize the results obtained at different levels of illumination. Thus, by referring to Table II we see that in Case A the retinal illumination in photons was approximately 2·10. In Case B it was 4·06, and in Case C it was 9·78. Cases B and C were approximately double Cases A and B. Curves *D* and *E* give the results obtained from varying the size of the retinal pattern. By referring to Table II, we see that in Case D the two fields were far apart so that the total distance amounted to 23° in terms of the visual angle subtended. In Case D, however, the total distance between the fields was small (they both fell within the rod-free area) as the visual angle subtended was only 2° 33′.

In no case, it will be noted, does the psychological subtractive effect parallel the subtractive effect of the stimulus. With low degrees of dimming of physical intensities, the subtractive effect of brightness is relatively greater. With high degrees of dimming of the physical intensities, the subtractive effect of brightness is relatively less. The maximal absolute subtractive effect occurred when the physical intensity was reduced from 3/4 to 7/8.

Table I gives the averaged data from which the curves in Fig. 2 were plotted.

VI. BEARING OF RESULTS UPON FECHNER'S PARADOX AND UPON F. ALLEN'S REFLEX EFFECTS.

Fechner's paradox is an observation to the effect that if the uniocular images differ in brightness the binocular brightness becomes considerably less than the brighter of the uniocular components. This subtractive effect was supposed to reach a maximum when the ratio of the two brightnesses was as 1 to 25. When, however, the ratio of the two brightnesses was as 1 to 1·5, the binocular brightness was said to be about equal to that of the brighter component. We believe that our statistical results are more accurate than those reported from the crude method usually employed in studying Fechner's paradox. According to our results, the maximal absolute subtraction occurred when the ratio of brightnesses was as 1 to 8 or 1 to 15. Our results do, however, corroborate the common observation that cutting off most of the brightness of a field has a greater absolute binocular subtractive effect than cutting out one field entirely.

On p. 248 we reported the observation that the summative and subtractive effect of different intensities of light upon each eye was present predominantly in consciousness only when there was a relative change (rise or fall) of brightness upon one eye. In this connection, we also observed that the task of equating the two fields after the darker field had been lightened was very much easier than the task of equating the two fields before the left field had been darkened. In other words, the organism seems to be more sensitive to a binocular summative effect than to a binocular subtractive effect.

We believe that Fechner's paradox is merely a special case of the absolute subtractive effect noted in our study. There are, however, relative effects, both summative and subtractive. There is, to be sure, no absolute arithmetical summation although, with low degrees of cut off, the observed effects are relatively greater than they should be if increases in psychological brightness were correlated perfectly with increases in physical intensity.

Our findings regarding the subtractive effect also harmonize with the important work of F. Allen[1] upon the reflex or induction effect of one eye upon the other as far as chromatic vision is concerned. He found as we did that the maximum absolute 'reflex' effect of one eye upon the other was obtained with very dim light and not with maximum dark adaptation. He showed clearly that the effect of stimulating one portion of one retina with a coloured light not only influenced neighbouring portions but also tended to depress the sensitivity of the opposite retina for light stimuli throughout the whole range of the spectrum.

VII. SUMMARY.

1. We have attempted to prove that there is a certain appreciable and measurable change in the binocular brightness observed when the illumination to one eye is altered.

2. Certain passages were taken from Sherrington's investigation upon this subject to indicate the inconclusiveness of his investigation. Important precautions are enumerated to show why Sherrington did not obtain results in agreement with those obtained in this study. Contrary to the conclusions of Sherrington, we maintain that the two eyes do function integratively as regards brightness, although we concur

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[1] A number of papers have appeared from Prof. Allen's laboratory, but most of his results may be found summarized in the *J. of Opt. Soc. of America*, XIII, Oct. 1926.

with him that increments and decrements of intensity cannot be perfectly correlated with increments and decrements of brightness.

3. We have pointed out that studies of binocular brightness should be carried out with an apparatus made to expose a standard field at all times to both eyes. Moreover, this standard should be of such nature that it can be varied and the variation measured in photometric units. In this way, when the light from the variable object to one eye is altered the standard field can be altered by the observer and a direct simultaneous measure of the difference in brightness accurately made.

4. Fechner's paradox is found to be a special case of the subtractive effect. In corroboration of Fechner's effect, we found that the maximal subtractive effect occurs with dim light and not with complete extinction.

5. Our results agree perfectly with the significant findings of F. Allen upon induced effects upon one retina resulting from stimulation of the other retina with coloured light, or with dark adaptation.

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MAXIMUM CAPACITY AND AVERAGE ACHIEVEMENT.

By S. WYATT.

I. *Maximum rate of working as a standard of measurement* (pp. 251–253).
  (*a*) *The problem* (p. 251).
  (*b*) *Unit chosen* (pp. 251–252).
  (*c*) *Reliability of unit chosen* (pp. 252–253).
II. *Applications to industrial procedure* (pp. 253–257).
  (*a*) *Individual differences* (pp. 253–254).
  (*b*) *Occupational differences* (pp. 254–255).
  (*c*) *Analytical considerations* (pp. 255–257).
III. *Discussion of results* (pp. 257–260).
IV. *Summary* (p. 260).

I. MAXIMUM RATE OF WORKING AS A STANDARD OF MEASUREMENT.

(*a*) *The problem.*

In almost all machine-feeding operations, output can be readily expressed as a percentage of the possible maximum. The latter is a definitely fixed standard which depends on the speed of the machine. In the numerous manual processes in industry, no such standard exists, and output is usually expressed in terms of average rate of working together with some measure of variability about this average. These values fail to show the extent to which activity is depressed by unfavourable elements associated with the conditions of work, and it is suggested that maximum achievement under the most favourable conditions, *i.e.* when the operative is actuated by suitable incentives and is free from fatigue, should be used for this purpose. The difference between maximum capacity and average achievement in the spell of work would then be a measure of the influences which are detrimental to productive activity.

(*b*) *Unit chosen.*

Obviously, the longer the period during which work is continuously performed, the lower will be the average rate of working in that period. The choice of a period to represent maximum attainment will be a somewhat arbitrary procedure, but must be long enough to neutralize the effect of a phenomenal spurt or errors of measurement, yet short enough to avoid the effects of fatigue or a weakening of the incentive to work. A period of 5 minutes seems to conform most closely to these requirements, since in that time it is usually possible to complete several units of output without feeling bored or fatigued. Providing the same unit of measurement is used when different individuals or processes are compared, the choice of 5, 10 or 15 minutes is relatively unimportant.

(*c*) *Reliability of unit chosen.*

Maximum performance in a 5-minute period appears to be very consistent from day to day. This belief is based on the results obtained from a number of industrial operatives engaged in making cigarettes by hand. Each operative was asked to work as quickly as possible at a time coinciding with the period of highest output (as shown by the work curves). The results obtained by adopting this procedure on five different occasions are given in Table I.

Table I. *Number of cigarettes completed by each worker during a 5-minute period when working at maximum speed.*

Worker	1st test	2nd test	3rd test	4th test	5th test	Average
A	23·0	23·5	23·3	24·0	23·2	23·4
B	23·5	23·0	23·9	23·2	23·9	23·5
C	27·0	27·0	27·5	26·5	27·1	27·0
D	25·0	24·2	25·8	26·0	25·2	25·2
E	26·2	28·0	26·8	27·4	26·2	26·9
F	28·5	27·2	29·4	28·1	28·7	28·4
G	27·5	28·1	28·0	27·2	27·6	27·7
H	24·0	25·0	25·5	24·8	24·1	24·7
I	24·5	23·5	25·0	24·8	24·3	24·4
J	28·5	30·3	29·5	30·0	29·2	29·5
K	28·8	28·5	28·7	28·8	29·5	28·9
L	24·0	22·5	23·2	23·0	23·7	23·3
M	30·2	29·8	29·0	30·2	28·5	29·5
N	21·0	21·5	22·6	22·1	22·8	22·0
O	23·2	23·0	23·8	23·3	23·0	23·3
P	26·2	27·0	26·5	26·7	26·0	26·5
Q	25·2	24·7	25·6	25·0	24·8	25·1
R	30·0	31·0	31·9	30·3	31·2	30·9
S	24·0	24·2	23·3	24·0	23·7	23·8
T	21·8	20·0	21·0	20·5	21·2	20·9

The results for each worker are very consistent, and indicate that the averages given in Table I are fairly representative of the maximum rate each worker is capable of attaining. This view is supported by the results obtained in a longer series of observations which was limited to six of the operatives included in Table I. Such results are given in Table II.

Table II. *Average number of cigarettes completed by each worker during a 5-minute period, together with the standard deviation of the separate readings from their average.*

Worker	No. of observations	Average	Standard deviation
A	29	23·2	0·56
K	20	28·9	0·61
L	26	23·2	0·57
Q	29	24·9	0·69
S	29	23·8	0·39
T	29	20·9	0·64

The results show that the average is not appreciably changed by an increase in the number of observations, but, if anything, becomes more variable as the series is prolonged. Thus a few readings appear sufficient to establish a reliable measure of maximum achievement.

II. APPLICATIONS TO INDUSTRIAL PROCEDURE.

(*a*) *Individual differences.*

The susceptibility of different individuals to similar conditions of work will be shown by the differences between maximum and average rates of working. Such results, obtained from the cigarette-makers, are given in Table III, and represent:

(i) The maximum rate per 5-minute period as given in Table I.
(ii) The average rate per 5-minute period throughout the day.
(iii) The difference between the maximum and average rates.
(iv) This difference expressed as a percentage of the maximum rate.

Table III. *Differences between maximum and average rates of working.* (*Cigarette-making.*)

Worker	Maximum rate	Average rate	Difference	
			Absolute	Relative
A	23·4	17·3	6·1	26·1
B	23·5	18·4	5·1	21·7
C	27·0	22·1	4·9	18·3
D	25·2	18·0	7·2	28·6
E	26·9	19·2	7·7	28·6
F	28·4	20·9	7·5	26·4
G	27·7	19·7	8·0	28·9
H	24·7	20·0	4·7	19·1
I	24·4	18·5	5·9	24·2
J	29·5	22·3	7·2	24·4
K	28·9	22·6	6·3	21·8
L	23·3	18·9	4·4	18·9
M	29·5	22·8	6·7	22·7
N	22·0	17·7	4·3	19·5
O	23·3	18·7	4·6	19·7
P	26·5	19·3	7·2	27·2
Q	25·1	18·9	6·2	24·7
R	30·9	24·9	6·0	19·4
S	23·8	17·5	6·3	26·4
T	20·9	16·4	4·5	21·5

The above results show that the absolute difference between maximum and average rates of working in the case of individuals exposed to similar conditions of work varies from 4·3 to 8·0 units while the relative difference ranges from 18·3 to 28·9 per cent. In this way operatives may be graded according to the extent to which they are affected by adverse influences associated with the productive process.

It is also apparent that the absolute difference between maximum and average rates of working tends to be greater in the quicker than in the slower workers. The coefficient of correlation (ρ) is + 0·65. Since, however, there is a general tendency for variations about a quantity to increase as the magnitude of the quantity increases, the correlation in question will probably be influenced by this tendency. The correlation between maximum rate of working and relative deviations from the maximum is still positive, though small, ρ equals + 0·16, but in other processes slightly higher coefficients have been obtained, viz.:

	ρ
Examining and packing (10 workers)	+ 0·84
Tobacco-weighing (10 workers)	+ 0·56
Soap-wrapping (6 workers)	+ 0·37

It accordingly appears justifiable to conclude that the quicker workers tend to be more susceptible than the slower operatives to unfavourable elements in the conditions of work.

At the same time it is evident that, in cigarette-making, a fairly close relation exists between the two series representing maximum and average rates of working. The coefficient of correlation (ρ) is + 0·88, consequently, in this process, those individuals who are capable of attaining the highest speeds also tend to produce the highest output under ordinary industrial conditions, and *vice versa*. In the other operations lower coefficients were obtained, viz.:

	ρ
Examining and packing...	+ 0·68
Tobacco-weighing	+ 0·34
Soap-wrapping	+ 0·26

(*b*) *Occupational differences.*

The maximum rate of working may also be used for the purpose of determining and comparing the total effect of adverse influences in different industrial processes. In this case group averages will be used, and the difference between the maximum and average rates of working in each group expressed as a percentage of the maximum. The following results (Table IV) illustrate the application of this procedure to different industrial processes.

Table IV. *Relative differences between maximum and average rates of working in different industrial processes.*

Process	No. of workers	Maximum rate	Average rate	% decrease
Cigarette-making	20	100	76·6	23·4
Examining and packing	10	100	83·4	16·6
Soap-wrapping	30	100	70·1	29·9
Tobacco-weighing	10	100	62·4	37·6
Rug-making	8	100	88·1	11·9

Subject to the limitations due to the small number of individuals in each group, the results show that the deviation from the maximum was least in the rug-making process. In this operation the operatives were under close supervision and the spell of work was 3 hours as compared with 4 hours in the other processes. The examiners and packers worked closer to their maximum than the cigarette-makers, presumably because the former occupation was more varied and consequently less boring or fatiguing than the latter. The large deviations from the maximum in the case of the tobacco-weighers was undoubtedly due to the fact that they were paid on a time-rate basis while the other workers were paid according to output produced. Soap-wrapping, in which the deviations from the maximum were also large, was a fairly strenuous operation since the workers had to keep pace with a conveyer. Talking was also difficult because of machinery noises, consequently boredom and fatigue were rather pronounced.

The above results give some indication of the total effect of negative influences in different industrial processes, and the method enables occupations to be classified according to the extent of these influences.

(c) *Analytical considerations.*

Differences between maximum and actual rates of working throughout the spell of work provide a basis for determining the nature and magnitude of particular factors which are detrimental to productive activity. The possibilities in this connection may be illustrated by reference to the results obtained in cigarette-making and tobacco-weighing (Fig. 1). These refer to individual workers and have been selected so as to illustrate the type of results obtained in the two processes.

In both curves, the highest rate of working attained by the operative is represented as 100 per cent., and the actual output recorded in consecutive 5-minute periods is expressed as a percentage of this maximum. The shaded area in each curve accordingly represents the total effect of factors which are detrimental to productive activity.

The most noticeable feature of the curves is the difference between the maximum and the highest rates of working observed under ordinary conditions of work. In cigarette-making, the operatives at some time or other during the day usually approached their maximum speed, but the highest observed rate in tobacco-weighing was generally well below

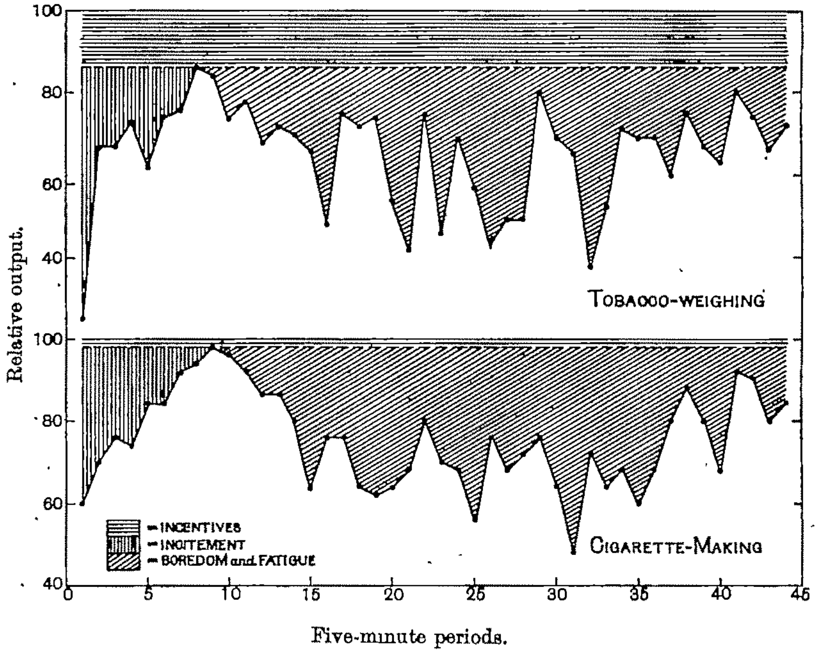

Fig. 1. Illustrating deviations from maximum efficiency and their probable causes.

the maximum. The most probable explanation of this difference appears to be connected with the monetary incentive in the two processes. The cigarette-makers were obviously anxious to earn as much as possible, while the tobacco-weighers seemed to work in a very leisurely manner. Thus, in this case, the general incentive to work is dependent upon the system of payment, and the weakness of this incentive is roughly proportional to the horizontally shaded areas in the curves.

The deviations from the broken line drawn through the highest point on the curve represents the effect of adverse influences of a temporary character. One of these is usually attributed to the process of 'warming-up' or incitement and is represented approximately by the vertically shaded area. The remaining deviations from the highest level of output recorded during the spell (obliquely shaded area) are usually explained in terms of boredom and fatigue. The variously shaded areas accordingly represent the approximate effect of the different negative incentives to activity, which, when expressed numerically as a percentage of the total loss, are as follows:

	Monetary incentives	Incitement	Boredom and fatigue	Total
Cigarette-making	2·0	3·6	19·3	24·9
Tobacco-weighing	14·0	3·7	17·4	35·1

These values, although given as illustrations of analytical possibilities, are typical of the results obtained in the cigarette-making and tobacco-weighing processes.

III. DISCUSSION OF RESULTS.

The validity of the arguments and inferences contained in this article depend upon the reliability of the measure of maximum achievement and its use as a standard of reference. Maximum performance over short intervals of time is a function of the psycho-physiological mechanism. It depends upon the ability of the mechanism to translate effort into work. In any individual, there is a fairly definite limit to momentary achievement. Thus a man may be able to register a pull of 100 lb. on a spring-balance, or to remember the names of six articles after seeing them flashed on a screen, and a repetition of the procedure at daily or weekly intervals will, in the practised individual, yield approximately similar results. In many respects an individual is like a machine driven from a storage battery, which, when fully charged and in the comparative absence of frictional and electrical resistance, is capable of producing maximum results. As the voltage falls, or resistances increase, the rate of working is also reduced. Significant deviations from the maximum rate are, however, in one direction, consequently the maximum, unlike the average rate of working, is exposed only to downward variations, and its reliability as a standard of measurement is correspondingly increased.

In general, therefore, maximum performance when recorded in the absence of fatigue and kindred factors is likely to be a fairly consistent and representative measure, and the results presented in Tables I and II support this view.

The applications of the procedure advocated in this article have some connection with the problem of measuring fatigue. As soon as psychology began to be treated experimentally attempts were made to discover a test for fatigue, and numerous psychological and physiological devices have been suggested for this purpose. The usually accepted definition of fatigue, as causing a diminished capacity for work, has tended to create the impression that all work-decrements are due to fatigue. Other factors, such as boredom and weak incentives have, in consequence, been comparatively neglected. Variations in the rate of working are usually the resultant of several positive and negative factors, and fatigue, although important, is not necessarily supreme. The total effect of adverse influences is shown by the difference between maximum capacity and average achievement, and this measure, whether applied to similar or different conditions of work, fulfils some of the requirements of a test for 'fatigue.'

When used in connection with individuals engaged in the same industrial process, it shows the variations in response to similar conditions of work, and provides a basis for further enquiry into the causes of these differences. It has been said, for instance, that the difference between maximum and average rates of working tends to be greater in the quicker than in the slower workers. This may be due to the fact, which has been noted during numerous investigations on repetition work, that the slower operatives are, as a rule, distinctly conscious of their disability, and attempt to overcome it by the expenditure of relatively greater effort. They are affected by the pace set by the quicker workers, while the latter, because of their superior speed, are more inclined to relax when such inclinations arise. It is highly probable, therefore, that the slower workers suffer more from fatigue than the quicker operatives, yet output curves may suggest the opposite tendency.

The differences in question may also have some connection with the amount of energy expended per unit of time by the quicker and slower workers. Worker R, for instance, was capable of producing 30·9 units while worker N produced only 22·0 units, and it seems reasonable to suppose that 30·9 cycles of activity consume more energy than 22·0 cycles. If this view is correct, worker R will expend more energy per unit of time than worker N (other things being equal), and, in consequence, will become relatively more exhausted as work proceeds[1]. That is, the deviations from maximum capacity will tend to be greater in the case of the quicker operatives.

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[1] This is undoubtedly true in the case of the *same* individual when working at higher speeds.

The relation between maximum capacity and average attainment throughout the spell of work will depend upon the relative strength of the incentives to activity and upon ability to endure the conditions of work. This relationship was found to vary in different processes, and was highest in the cigarette-making operation. The operatives in question were the keenest and most industrious group of workers observed over a period of 10 years. They were older than the average women employed in industry and seemed particularly anxious to earn as much as possible. When all the operatives in a group are actuated by strong incentives the possibilities of a close relation between maximum and average rate of working are correspondingly increased.

The tobacco-weighers, who received a time wage, represent the other extreme in this respect. While some were comparatively industrious, others appeared to produce just enough to enable them to keep their jobs in a department controlled by a particularly charitable foreman. Deviations from the maximum were accordingly much greater in some cases than in others, and the low coefficient (0·34) reflects this state of affairs.

As regards the soap-wrappers, the low correlation obtained (0·26) appears to be due to a number of causes. In the first place the workers, who were comparatively young, were taken separately to an adjoining room when being tested for maximum efficiency and some of the best operatives seemed to be adversely affected by this procedure. In the other processes maximum capacity was measured under ordinary conditions of work. Secondly, the slower workers talked less and took fewer voluntary rests than the quicker operatives and consequently deviated relatively less from their maximum speed. Thirdly, a few of the physically weaker operatives were less able to endure the additional strain imposed by trying to keep pace with the conveyer.

A broad survey of industrial processes along the lines suggested in this article would disclose factors of general interest and importance, and indicate the most profitable lines for further research. In the course of time it should be possible to determine the normal amount of deviation to be expected in different forms of industrial activity so that whenever this amount is exceeded the causes can be ascertained.

A standard of possible achievement is a necessary prelude to the determination of the particular factors responsible for reduced efficiency. Such an analysis, when treated quantitatively, is necessarily somewhat speculative, since it is often difficult to say where one factor begins and another ends. Since, however, the results of recent experiments have increased our knowledge in this direction, the possibilities of accurate measurement have been correspondingly increased.

One of the chief problems in modern industry is the provision of conditions which will facilitate the economical release of energy, and the degree of approximation to such conditions will be shown by the difference between maximum capacity and average achievement.

IV. SUMMARY.

1. It is suggested that the difference between maximum capacity recorded under the most favourable conditions and average achievement observed throughout a spell of work is a measure of the combined influence of factors which are detrimental to productive activity.

A suitable measure of maximum capacity is the highest rate of working attained in a period of 5 minutes. This value is very consistent in the case of the same individual observed at corresponding times on different days.

2. The application of this method to industrial procedure can be used to show

(*a*) Individual susceptibility to similar conditions of work.

(*b*) The total effect of unfavourable factors in different industrial processes.

(*c*) The approximate influence of particular factors responsible for reduced activity.

3. When applied to the processes considered in this article, there is evidence to show that

(*a*) One worker may be almost twice as susceptible as another to unfavourable elements associated with the conditions of work.

(*b*) The quicker workers tend to deviate more than the slower operatives from their respective highest speeds.

(*c*) An imperfect relation exists between maximum and average rates of working, yet when all the workers in a group are actuated by a powerful incentive the degree of imperfection may be very slight.

(*d*) The difference between maximum and average rates of working may vary from 12 to 38 per cent. in different processes.

(*e*) A time-rate system of payment is much less effective than a piece-rate and may cause a reduction in output amounting to approximately 12 per cent.

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II. THE PROBLEM STATED.

Having now had some 14 years' inspectorial experience in East-end London Schools, with the results and papers of the London County Council Preliminary Examination for Junior County Scholarships coming before me every 6 months, and hearing the frequently expressed views of the teachers, I could scarcely have refrained from arriving at some more or less reasoned opinions about the relative characteristics of Jewish and Christian children. Further, for some 20 years past, I have given sets of my own 'reasoning tests' to whole schools in different parts of London the results of which, from time to time, have lent support to the opinions of the experienced teachers who had, or had had, charge of schools of Christian or Jewish children or of Jewish and Christian children mixed. Those opinions, confining myself to intellectual characteristics, were and are: (1) that Jewish children, age for age, are more intelligent than Christian children; (2) that Christian children are somewhat more proficient in manual work than Jewish children; (3) that there is a greater difference between the mental powers of the best and the worst of the Jewish than there is between the best and the worst of the Christian children; (4) that the Jewish girls show bigger differences from Jewish boys than Christian girls from Christian boys. For some years past, I had given sets of my reasoning tests in East-end schools, among others, and there is no doubt that, in schools attended by Jewish children, the results are decidedly higher than those for Christian children in adjacent, or even in what appear to be the same neighbourhoods. It became obvious, at quite an early stage of my inspectorial work, that often, when Jewish and Christian children attended the same school, the Christian children were generally of poorer and lower social type than the Jews. In school E, for example, the Christians came from two very poor and mean streets in the immediate vicinity of the school, where no Jewish children lived. In school F, the Jewish children came from the Whitechapel side of the school, and the Christians from St George's-in-the-East, and there, too, the differences of ability were so marked that they were a matter of general comment. Further examples of this kind could be given. But it may well be asked, "Are there not schools in which the whole of the pupils in attendance are Jewish, why not compare them with schools in which all the pupils are Christians?" The issues are not so simply decided. For, no scholastic proficiencies, no 'mental or intelligence tests' are uninfluenced by teaching, and it would be necessary to have the same or equal teachers if the results were to have more than an inspectorial value. We must have the pupils, if we are to compare them satisfactorily, race by race, age by age, taught in the same school, with the same syllabuses of instruction, by the same methods, class by class, and by the same teachers. We may, of course, indeed, we often do, in a rough practical way, say, "Such a Christian school is of the same social class as such a Jewish school, and the teaching assessments of the staff are approximately equal, what are the comparative results in this or that 'mental-test' or test of attainment in these conditions?" But one can never feel any real security; for if, as I believe, a close equality in instruction and training are necessary before the results of any 'mental test' can have exact comparative value as to natural ability, we must avoid these large-scale observations in which, of necessity, the relevant differences, other than those with which we are specially concerned, remain unknown and obscured.

III. WHAT IS MEANT BY THE TERM 'SOCIAL CLASS'

By 'social class' I mean something rather more inherent than 'home circumstances' though less inherent than race. Some of the criteria which I shall employ *are* criteria of home circumstances. But 'he ain't no class' is a current term of reproach which has no direct relation to prosperity or poverty. Some investigators have used the rental of the family as an index number, which has yielded valuable results. Some have used the occupation of the parent as a guide. It is believed that Jewish families are larger than Christian families within the same social ranks. Some of the children may be at work and contributing to the family exchequer. A high rental with a low figure of individual accommodation may indicate a lower social class than a smaller rental and a smaller family. There is little doubt that good social class does lead ordinarily to a relatively high payment for housing accommodation, more than for clothing or food. The occupation of the parent is useful also, but it is not decisive. So I obtained through the head teachers, and the experienced members of their staffs, and the knowledge of a local attendance officer, particulars of family rental, the number of rooms occupied, the number of adults living in them, the number of children living at home, the occupations of the parents, and those other indications of 'class' or 'status' which are known to teachers and to local workers. Then the head master of the boys' and the head mistress of the girls' school made independent estimates, giving the marks, 4, 3, 2, 1, or 0 according to the aggregate of the social class conditions.

Since many of the children had brothers and sisters attending the same school, there was an opportunity of collaboration and comparison in the marks allotted to the same and other families.

There is one important issue which needs mention. A larger proportion relatively, year by year, of Jews than Christians had obtained scholarships, attended central schools (which happens at 11 plus years of age), or had become fee-paying pupils at secondary schools.

IV. THE TESTS, THEIR ADMINISTRATION AND MARKING.

I used some sets of my own reasoning tests as indications of natural rational intelligence[1]. The tests used in these two schools were Sets *A* and *E*. The methods of marking the problems of Set *A* are fully set out in the *Journal of Experimental Pedagogy* for December, 1921. Examples of the answers, and the marking of the problems of Set *E* have not yet been published, but the questions have been much used over a period of years, and modified in accordance with the results from time to time. Every paper was marked by me for both sets of tests in both schools, with a *viva voce* reference in every case in which the child's apparent meaning was not clear. The second set of tests was given one week after the first. As a measure of manual dexterity, we used the handwriting which was done in writing out the answers to the questions in reasoning. The marks for each set were 4, 3, 2, 1, or 0 both in reasoning and handwriting.

V. THE VALUE OF SCHOOL CLASSES AND SCHOOL GRADINGS.

A valuable means of estimating the 'standard' or 'grade' of scholastic attainment will be found in the school grading into standards, of which there are seven, the seventh being the highest. They depend on an aggregate of marks in terminal examinations, and not on intelligence tests. Let us first see what light is thrown on the relative abilities of Christian and Jewish boys and girls when the school standards, their ages, their reasoning marks, their writing marks, and their marks for social class are compared in sets of averages.

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[1] "The Transfer of Improvement in Reasoning in School-Children," this *Journal*, April 1923, XIII (4).

VI. RESULTS.

Table I. *Showing school standards of Christians and Jews, their ages, reasoning and writing marks, and marks for social class compared.*

Boys.

School standards	No. of boys		Av. age in years and months				Av. mark for reasoning		Av. mark for handwriting		Av. mark for social class	
	C.	J.	C.		J.		C.	J	C.	J.	C.	J.
			y.	m.	y.	m.						
VII	9	21	13	4	12	6	3 28	3·55	2·67	2·71	2·22	2 71
VI	15	21	12	6	11	10	2 97	2·90	2·67	2·74	2·40	2·62
V *a*	14	25	11	11	11	3	2·71	2·80	2·71	2·86	1·57	2·32
V *b*	12	15	12	1	11	3	2·17	2·10	2 63	2·30	1·75	2·87
IV	21	15	10	11	10	3	1·74	1·93	2·38	2·43	1·86	3·00
III	14	19	9	9	9	5	1·03	1·66	1·25	1·79	2·00	2·84
II	16	14	9	3	8	0	0·63	0·61	2 03	1·86	1·63	3·07
I	17	16	7	7	7	8	0·03	0·16	0·88	1·22	1·53	2 44

C. = Christians. J. = Jews.

It is evident that in standard of attainments, the Jewish boys are considerably in advance, age for age, of the Christian children, for they have reached their respective standards, as a rule, indeed almost invariably, at an earlier age, in some cases at a much earlier age. The marks for reasoning show a close correspondence between the Jewish and Christian boys within the same school standards, as do also those for handwriting, as they should do. In social class the Jews show a marked superiority, and the question may very fairly be raised whether the whole difference in standard of scholastic attainment may not be due to the factor of social class rather than that of race.

Let us now cut across the classification by school grading, and classify the boys according to age-groups.

Table II. *Showing age-groups of Christians and Jews, their reasoning and writing marks, and marks for social class compared.*

Boys.

Age-groups in years	No. of boys		Av. age in years and months				Av. mark for reasoning		Av. mark for handwriting		Av. mark for social class	
	C.	J.	C.		J.		C.	J.	C.	J.	C.	J.
			y.	m.	y.	m.						
14	3	3	14	0	14	4	3·17	3·00	2·50	2 67	1·00	2·33
13	16	18	13	5	13	6	2·97	2·75	2·84	2·25	2·06	2·39
12	24	19	12	3	12	4	2·46	2·76	2·50	3·00	1·92	2·42
11	16	22	11	5	11	5	1·59	2·57	2·59	3 00	1·38	2·56
10	19	28	10	5	10	5	1·71	2 59	2·27	2·30	2·00	2·89
9	9	19	9	5	9	5	1·50	1·92	1·78	2·18	2·00	2·74
8	15	18	8	5	8	5	0·57	1 11	1·40	1 86	2 07	2·94
7	15	17	7	6	7	6	0·13	0·32	1·00	1·38	1·67	2·71
6	1	2	6	11	6	11	0·50	0·75	1·00	0·75	2·00	3·50

Nearly all the boys of 14 had left the school, and of the normal group of 12 and 13-year old children, some of the abler children, predominantly Jews, had obtained scholarships to secondary schools or were attending central elementary schools. Of the 7-year old group, a very few were in the infants' department; and, of course, only a very few of the abler 6-year old children had been promoted from the infants' school. With these facts in mind, it is apparent that the Jewish boys have scored in reasoning, age for age, all along the line. They are also rather better writers; but they are much superior in social class in every age group. And again the question arises, if social class is a factor in these results, may not the whole superiority exhibited by the Jewish boys be due to this factor, and not at all to race?

Let us now turn to the girls' department of the same school, which is, of course, a separate school in conduct and organization.

Table III. *Showing school standards of Christians and Jews, their ages, reasoning and writing marks, and marks for social class compared.*

Girls.

	No. of girls		Av. age in years and months				Av. mark for reasoning		Av. mark for handwriting		Av. mark for social class	
School standards	C.	J.	C.		J.		C.	J.	C.	J.	C.	J.
			y.	m.	y.	m.						
VII	5	13	13	2	13	2	2 50	3·04	2·80	2·85	2·40	2·62
VI	7	24	12	2	12	3	2·86	2·29	3·21	2·83	2·43	2·63
V *a*	19	17	11	10	11	8	2·50	2·53	2·69	2·88	2·00	2·47
V *b*	15	17	12	3	11	4	2·43	2·44	2·63	2·21	1·40	2·41
IV *a*	13	25	11	6	10	5	1·81	2·22	2·85	2·96	1·23	2·32
IV *b*	18	15	11	0	10	2	1·78	1·90	2·42	2·20	1·44	2·40
III	12	21	10	1	10	0	0·93	1·33	2·42	2·33	1 33	2 57
II	18	18	8	3	8	3	0·58	0 89	2·69	2·39	1·89	2·06
I	16	19	7	5	7	4	0·09	0·26	1·84	1·95	1·25	1·79

Again we see that in standard of scholastic attainment, measured by their school grading, the Jewish children are in advance of the Christian children, for they have, almost invariably, attained their school standards at an earlier age, in some cases at a much earlier age. The marks for reasoning show, except in the case of the girls of Standard VI, a balance of advantage in favour of the Jews, even though the children are in the same scholastic standards. In handwriting the balance of advantage oscillates; it can hardly be said that there is any superiority either way. In social class the Jewish children show a marked superiority all along the line.

Let us now classify in accordance with age-groups and disregard the school grading into standards of attainment.

Table IV. *Showing age-groups of Christians and Jews, their reasoning and writing marks, and marks for social class compared.*

Girls.

Age-groups in years	No. of girls		Av. age in years and months				Av. mark for reasoning		Av. mark for handwriting		Av. mark for social class	
			C.		J.							
	C.	J.	y.	m.	y.	m.	C.	J.	C.	J.	C.	J.
14	0	2	—		14	1	—	3·00	—	2·50	—	2·50
13	16	20	13	7	13	7	2·22	2·73	2·59	2·88	1·13	2·40
12	21	19	12	5	12	4	2·21	2·39	2·88	2·58	1·52	2·10
11	19	26	11	6	11	5	2·79	2·35	2·58	2·62	1·63	2·27
10	24	31	10	6	10	5	1·96	2·13	2·60	2·45	2·04	2·42
9	12	24	9	6	9	7	1·25	1·83	2·54	2·69	1·75	2·71
8	7	23	8	5	8	5	0·93	1·43	2·64	2·39	1·57	2·74
7	20	18	7	6	7	5	0·23	0·31	2·10	1·92	1·50	1·78
6	4	6	6	10	6	10	0·13	0·33	1·86	2·58	1·50	2·16

Nearly all the girls of the 14-year old group have left the school, and some members of the higher age-groups have obtained scholarships to secondary schools or are attending central elementary schools, not so many as in the case of the boys' department, though, of those who are willing thus to extend their school education, the majority are Jewish. The 7-year old group is not quite complete, there are some 'sevens' in the infants' school. Only a very few of the abler 6-year old girls are, of course, to be found in the girls' school.

Making due allowances, or even without them, it is apparent that in the case of every age-group the balance of marks for reasoning is distinctly on the Jewish side. This is not the case with the marks for handwriting, where the balance of advantage shifts from side to side, neither Jews nor Christians, age for age, appearing to possess an advantage. In marks for social class the Jews have a distinct advantage in every case, and again it would seem that the whole superiority, where it exists, of the Jewish children may be the result of a social class factor and not of racial superiority. It is, perhaps, worth noting that, comparing the work of the boys with that of the girls, the boys, as usual, have the advantage in reasoning, and the girls have the advantage in handwriting, especially in the lower grades or standards. But the object of this research is not, except subsidiarily, a comparison of the work of boys and girls—we should require a guaranteed equality of pedagogical influences to be quite certain as to that issue—but rather the influence of a social class factor on the standard of school attainment of Christian and Jewish boys and girls, with some sidelights on that influence, which are much less securely based, of rational intelligence, so far as that is indicated by the results of two sets of reasoning tests, and on manual ability, so far as that is indicated by the current handwriting of the children.. Of the general importance of a 'social class' factor it is hardly necessary to speak, since now, even in official reports, it is so generally accepted that, before proceeding to a detailed account of the work done in the school, they frequently set out with what is really a social class description of the school, in the light of which either, favourably or unfavourably, the subjoined account must be read. But, though the influence of this 'social class' factor is now generally admitted, I have not yet shown in any detailed way the probable amount of its influence in the case of these Christian and Jewish boys and girls. To do this, I must give a numerical assessment for the school standards, and I have done so by allowing 7 marks for the seventh, the highest standard, 6 marks for the sixth standard, and so on, downwards. Then the correlation coefficient or *r* has been found between the social class mark, which, it will be remembered, ranges from 4 to 0, placing the children in five classes or groups. It has been worked out individually in all cases, omitting the 14-year old and the 6-year old children for obvious reasons. The numbers in some of the other cases are small (they will be found in Tables II and IV) and some allowance must be made for that.

Table V. *Showing for each age-group of boys and girls, Christian and Jewish children separately, the correlation coefficient between their school grading and their social class.*

Age-groups in years	Boys		Girls	
	Christians	Jews	Christians	Jews
13	+0·52	−0·11	+0·61	+0·33
12	+0·43	+0·38	+0·51	+0·67
11	+0·56	+0 08	+0·50	+0·19
10	+0·21	+0·21	+0·54	+0·23
9	−0·12	+0·15	−0·06	+0·69
8	+0·44	+0·34	+0·44	+0·51
7	+0·55	+0·26	+0·50	+0·16

There is, almost invariably, a fairly high positive correlation between the two series; such negative correlations as exist are very small, and, making allowance for all small coefficients, both positive and negative, we can accept the table as indicating clearly a positive relation. That social class is a causal factor has been elsewhere and otherwise demonstrated; this table only indicates, in a rough way, the extent to which it may, in this case, be relied on for each age-group of Jews and Christians. It is fairly certain that the influence of social class is considerable; and that it is on the side of the Jewish children, both boys and girls, we already know. It may account for, perhaps it may even more than account for, the superiority of the Jewish children, age for age. Is there any residue of superiority due to race at all? If the children of the same age-group, and with the same social-class marks are shown in comparative tables, we ought to find an answer to that question. This I now proceed to set out.

Table VI. *Showing the children of the same marks for social class, and of the same age, compared in scholastic attainment, in reasoning and in handwriting.*

Boys.

	JEWS.				CHRISTIANS.			
Mark for social class	No. of boys	Average mark for school grading	Average mark for reasoning	Average mark for hand-writing	No. of boys	Average mark for school grading	Average mark for reasoning	Average mark for hand-writing
13-year old group.								
4	1	7·00	4·00	3·00	2	6·00	2·75	3·75
3	8	5·88	2·50	2·25	4	7·00	3·50	2·88
2	7	6·43	2·79	2·43	4	6·25	2·75	2·50
1	1	7·00	3·50	1·50	5	5·60	2·80	2·70
0	1	6·00	2·50	2·00	1	5·00	3·00	3·00
12-year old group.								
4	5	6·30	3·00	2·40	1	6·00	3·50	2·00
3	5	6·40	3·30	3·50	7	5·43	2·64	2·57
2	4	5·50	2·13	3·63	8	5·13	2·75	2·38
1	3	6·00	2·23	2·17	5	4·80	1·80	2·70
0	2	5·00	2·75	3·25	3	4·00	2·00	2·50
11-year old group.								
4	6	5·50	2·33	2·83	0	—	—	—
3	7	5·29	2·57	3·21	2	4·50	1·50	3·00
2	3	5·00	2·33	2·50	6	5·00	2·00	2·92
1	5	5·40	2·80	3·40	4	3·75	1·38	2·25
0	1	5·00	3·00	2·50	4	3·25	1·25	2·25
10-year old group.								
4	12	5·17	2·92	2·38	3	4·67	2·00	2·83
3	10	4·20	1·95	2·30	2	3·50	2·75	2·75
2	2	5·50	3·75	2·25	8	3·88	1·50	2·06
1	2	5·00	3·00	2·00	4	4·00	1·63	2·00
0	2	4·00	2·25	2·25	2	3·50	1·25	2·50
9-year old group.								
4	6	4·00	2·08	2·50	1	4·00	3·50	3·00
3	5	4·00	1·70	2·40	3	3·00	1·17	2·00
2	6	3·83	2·00	1·92	2	3·00	1·25	1·25
1	1	3·00	0·50	1·50	1	4·00	2·50	1·50
0	1	4·00	3·00	1·50	2	3·50	0·75	1·50
8-year old group.								
4	9	2·33	1·33	1·72	3	2·67	1·17	1·50
3	3	2·33	1·50	2·33	1	2·00	0·00	2·00
2	3	2·33	0·83	1·83	7	2·00	0·71	1·29
1	2	1·50	0·50	2·25	2	1·50	0·25	1·75
0	1	1·00	0·00	1·00	2	2·00	0·50	1·25
7-year old group.								
4	4	1·75	0·75	1·88	1	2·00	1·50	2·00
3	7	1·57	0·36	1·64	1	1·00	0·00	1·00
2	4	1·00	0·00	0·63	6	1·17	0·08	1·00
1	1	1·00	0·00	1·50	6	1·00	0·00	1·00
0	1	2·00	0·00	0·50	1	1·00	0·00	0·00

Table VI (*contd*)

Girls.

	Jews.				Christians.			
Mark for social class	No. of girls	Average mark for school grading	Average mark for reasoning	Average mark for hand-writing	No. of girls	Average mark for school grading	Average mark for reasoning	Average mark for hand-writing
				13-year old group.				
4	2	6·00	3·50	3 00	—	—	—	—
3	8	6·13	2·56	3·06	2	6·50	2·50	3·25
2	4	6·50	3·25	3·13	4	5·50	2·50	2·38
1	6	5·00	2·33	2·42	4	5·00	1·75	2·50
0	0	—	—	—	6	4·67	2·25	2·58
				12-year old group.				
4	2	6·00	2·50	2·50	—	—	—	—
3	4	6·00	2·50	2·88	1	7·00	2·00	4·00
2	9	5·78	2·28	2·61	10	5·30	2·45	2·85
1	2	5·50	2·50	2·25	9	4·67	2·17	2·67
0	2	4·00	2·50	2·25	1	4·00	0·50	4·00
				11-year old group.				
4	2	5·50	2·10	2·50	—	—	—	—
3	9	5·22	2·39	2·89	3	4·67	2·33	2·33
2	10	4·90	2·25	2·40	8	4·88	2·63	2·50
1	4	5·00	2·50	2·50	6	3·83	1·42	2·75
0	1	5·00	3·00	3·00	2	4·00	1·50	2·75
				10-year old group.				
4	5	4·75	1·80	2·70	2	6·00	3·50	2·50
3	9	4·56	2·67	2·61	6	4·50	2·33	2·67
2	13	4·08	1·81	2·23	10	4·00	1·55	2·25
1	2	4·00	3·25	2·25	3	3·33	1·83	3·17
0	2	4·00	1·50	2·75	3	4·00	1·67	2·83
				9-year old group.				
4	7	4·29	2·43	3·00	2	3·50	2·00	2·50
3	7	3·71	2·07	2·93	1	2·00	1·00	3·00
2	7	3·57	1·64	2·50	3	2·67	0·83	2·50
1	2	2·00	0·25	1·75	4	3·00	1·13	2·63
0	1	2·00	0·50	2·00	2	3·50	1·50	2·25
				8-year old group.				
4	5	3·40	1·60	2·10	0	—	—	—
3	9	2·67	1·61	2·50	1	3·00	2·50	2·50
2	7	2·57	1·29	2·29	2	2·00	0·00	3·25
1	2	2·00	0·75	3·00	4	2·25	1·00	2·38
0	0	—	—	—	0	—	—	—
				7-year old group.				
4	1	1·00	2·00	3·50	0	—	—	—
3	2	1·50	0·00	1·50	3	2·00	0·00	2·50
2	9	1·33	0·33	2·00	7	1 43	0·51	2·36
1	4	1·25	0·00	1·88	7	1·43	0·07	2·07
0	2	1·00	0·25	1·25	3	1·00	1·00	1·17

Comparing those of the same social class in each case, out of 34 possible comparisons, there are 24 cases in which the average school grading of the Jewish boys is superior to that of the Christian children, 3 in which it is the same, and 7 in which the Christian boys are superior.

Comparing the average marks for reasoning, there are 22 cases in which the Jewish boys are superior, 2 in which they are the same, and 10 in which the Christian children are superior. Comparing the average marks for handwriting, there are 16 cases in which the Jewish boys are superior, 3 in which they are the same as the Christian boys, and 15 in which the Christian boys are superior.

Turning now to the girls, out of 28 possible comparisons between children of the same social class and of the same age-groups, there are 13 cases in which the Jewish girls have superiority in school grading, 4 cases in which the average grading is the same, and 11 cases in which the Christian girls show superiority. In the average marks for reasoning, there are 17 cases in which the Jewish girls show superiority, 1 in which they are the same, and 10 in which the Christian children are superior. In handwriting there are 7 cases in which the Jewish girls are superior, 2 in which they are the same, and 19 in which the Christian girls are superior.

Summing up, it is difficult to resist the conclusion that Jewish children, both boys and girls, of the same age and social class as Christian children, are superior in their standard of school attainment, and, as far as our tests are indicative, the Jewish boys decidedly so in reasoning, the girls much more doubtfully, if at all, whilst in handwriting the superiority of the Jewish boys is very, very doubtful, and the Jewish girls are definitely inferior to the Christian girls. The differences in intelligence between Jews and Christians are greater in the male than in the female sex. The general contentions of the teachers are so far confirmed, namely, that Jewish boys and girls of elementary school age are more advanced for their ages and more intelligent, but so far as handwriting is indicative of manual dexterity, the Jewish boys appear about the same, and the Jewish girls definitely inferior to the Christian girls.

I have already given the correlation coefficient between the social class of the children and their school grading for each age-group of Christians and Jews separately. I now propose to show the r coefficient between their social class and the only intelligence tests which I have used, namely, the two sets of reasoning tests. And finally, I shall give the coefficients between the results of the reasoning tests and the results of the handwriting exercises, and I shall give the coefficients of variability (found by dividing the standard deviation by the average) of social class, school grading, reasoning and handwriting. These last comparative figures will enable us to check the current belief as to the greater variability of the Jewish than of the Christian children, which is so often asserted by teachers.

Table VII. *Showing the correlations (r coefficients) between social class and reasoning for each age-group of Jews and Christians, boys and girls separately.*

Age-groups in years	Boys		Girls	
	Christians	Jews	Christians	Jews
13	+0·21	+0·02	+0·20	+0·26
12	+0·37	+0·37	+0·36	+0·03
11	+0·24	−0·22	+0·52	−0·26
10	+0·30	+0·06	+0·44	+0·07
9	+0·42	+0·01	+0·15	+0·60
8	+0·32	+0·34	+0·26	+0·32
7	+0·65	+0·42	+0·23	+0·39

The coefficients are nearly all positive; they are generally lower in the case of the Jewish than of the Christian children: they are, on the whole, not so high as those between social·class and school grading, which depends not on reasoning as such, but on an aggregate of marks for scholastic subjects, some only of which, as, for example, problematic arithmetic, involve reasoning.

Table VIII. *Showing the correlation coefficients between the results of the reasoning tests and the exercises in writing for Christians and Jews, for each age-group, boys and girls separately.*

Age-groups in years	Boys		Girls	
	Christians	Jews	Christians	Jews
13	+0·18	+0·03	+0·03	+0·32
12	−0·09	+0·04	−0·05	+0·17
11	+0·63	+0·28	−0·22	+0·13
10	+0·24	+0·18	+0·22	+0·17
9	+0·67	+0·28	+0·27	+0·24
8	−0·14	+0·15	+0·02	+0·19
7	+0·66	+0·37	+0·32	+0·58

The correlation coefficients are, as before, nearly all positive; but, with some outstanding exceptions, are decidedly low.

Table IX. *Showing the coefficients of variability in school grading, social class, reasoning and writing for each age-group of Christians and Jews.*

Age-groups in years	School grading				Social class			
	Boys		Girls		Boys		Girls	
	C.	J.	C.	J.	C.	J.	C.	J.
13	0·14	0·15	0·17	0·20	0·54	0·38	1·00	0·44
12	0·21	0·15	0·20	0·13	0·58	0·54	0·47	0·52
11	0·26	0·18	0·16	0·16	0·71	0·47	0·56	0·39
10	0·27	0·21	0·21	0·23	0·59	0·40	0·55	0·42
9	0·20	0·20	0·37	0·25	0·67	0·42	0·72	0·41
8	0·32	0·41	0·17	0·26	0·57	0·45	0·44	0·33
7	0·31	0·33	0·33	0·31	0·55	0·40	0·60	0·56

Table IX (*contd*).

Age-groups in years	Reasoning				Writing			
	Boys		Girls		Boys		Girls	
	C.	J.	C.	J.	C.	J.	C.	J.
13	0·18	0·32	0·27	0·30	0·25	0·30	0·23	0·24
12	0·38	0·25	0·32	0·17	0·32	0·23	0·21	0·19
11	0·56	0·31	0·38	0·22	0·35	0·27	0·15	0·23
10	0·53	0·39	0·45	0·43	0·35	0·39	0·23	0·28
9	0·67	0·58	0·77	0·56	0·39	0·27	0·20	0·26
8	*1·17	1·00	1·11	0·50	0·43	0·32	0·23	0·25
7	*4·00	2·00	2·15	2·00	0·70	0·50	0·29	0·42

* The high variability in reasoning in the lower age-groups *may* be an indication that the questions are not graded closely enough, and are not adequate tests of reasoning at these ages.

It is a conservative conclusion that, disregarding the few children who have obtained scholarships or are attending Central schools, of which more than a fair proportion are Jewish, and disregarding the very few children who have been accepted in schools for the mentally defective, and have been so allocated at the statutory examinations therefore, and considering the children remaining in the ordinary elementary school, the variability of the Jews is slightly less than that of the Christians in school grading, is less in social class and in reasoning, and is less in writing on the whole, as far as the boys are concerned, and more in writing as far as the girls are concerned. There is apparently no justification for the ordinary opinion that Jewish children are more variable intellectually.

VII. Summarized conclusions.

1. That Jewish children attending East-end Elementary Schools are superior intellectually to Christian children.
2. That much of this superiority is due, not to race, but to a superiority of social class.
3. That, when children of the same social class are compared, the Jewish children still show some intellectual superiority.
4. That the difference between the Jewish boys and Christian boys is greater than that between the Jewish girls and Christian girls.
5. That in manual dexterity, so far as is shown by handwriting, Jewish and Christian boys are more nearly alike, and Christian girls are superior to Jewish girls.
6. That the variabilities within the same age-groups of the Jews are somewhat less marked than those of the Christians, except in the case of the writing of the Jewish girls.

(*Manuscript received* 5 *June*, 1928.)

THE MEASUREMENT OF INTELLIGENCE IN A RURAL AREA.

BY J. B. RUSSELL.

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VII. *Discussion* (pp. 293–295).

I. INTRODUCTION.

IN 1920–1 Godfrey H. Thomson prepared a Group Mental Test for use in Northumberland, and the results of his researches were published in this *Journal*[1].

In 1925 the same test was used, with the concurrence of the Local Education Authorities, to effect a mental survey of two age groups in an Eastern Administrative County and the central Borough.

In the interval, 1925–8, some further information has been obtained by a study of:

(*a*) the abilities of children whose parents follow agricultural occupations;

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[1] This *Journal*, 1921, XII (3), and 1923, XIV (2).

(*b*) the ancestry of a selected group;
(*c*) the replies of teachers to questionnaires sent out in 1927, two years after the date of the test.

II. METHODS.

Object of the test.

The primary object of the test was to make a first selection of gifted children in attendance at elementary schools in the areas concerned.

It was anticipated also that the mental test might reveal at least one of the reasons why many elementary schools rarely submit candidates for the examination in English and Arithmetic, on the result of which free places are allotted in secondary schools.

For geographical reasons, it was obvious that the tests would have to be applied in the elementary school under the supervision of the teachers. To secure uniformity of treatment for all examinees, it appeared desirable that little or nothing should be left to the discretion of supervisors, that the actual working of the test should not take more than about an hour, and that the marking of the papers under supervision should be expeditious.

Northumberland test, No. 1, appeared in every way to meet these requirements and its final selection was not unconnected with the circumstance that between 1923–5 I had used the test with increasing confidence for purposes of diagnosis in individual cases.

Preliminary arrangements.

The authorities sent two forms to each school, on one of which head teachers entered the following information in columns:

1. Name:
2. Date of birth:
3. Age in years and months on March 31st, 1925:
4. Head teacher's estimate of ability on a five-point scale, A–E:
5. Class on March 31st, 1925, before promotion.

On the same form, columns headed 'Mental Age,' 'Age,' 'I.Q.' were left vacant for the use of assistant examiners.

On the day preceding the test proper, a 10 minutes' preliminary practice test was worked.

On April 2nd, 1925, all children of 10 and 11 years of age in the county and the borough worked through Northumberland test, No. 1, between 10 and 11 a.m., and the papers were returned to head-quarters on the same day.

The time allowance was found to be ample, and nearly all the children completed as much as they were able to do within the limited period.

In all, 3379 children, distributed as follows, worked through the test:

	County	Borough
Boys, aged 10	770	111
Girls, ,, 10	745	116
Boys, ,, 11	728	92
Girls, ,, 11	715	102
	2958	421 = 3379

Marking and tabulation.

The worked booklets were marked in one room, under my supervision, by a team of teachers selected from the elementary and secondary schools, to all of whom I am greatly indebted[1].

For transmission to the schools, the I.Q. marks for each examinee were entered on a record form on which the names had previously been written. The form provided a column for the father's occupation as well as spaces for the entry of the names of those children, who although they did not reach the score of I.Q. 115 +, fixed as the minimum mark for the first selection of candidates, were specially recommended (*a*) by head teachers, or (*b*) desired by their parents to sit for the subsequent attainment examination in English and Arithmetic.

In the majority of cases, no names were entered under (*a*).

In Table I the massed results of the mental test are expressed as percentages in terms of Intelligence Quotients, and in Tables II A and II B further analyses of the scores are made.

A comparison of columns (1) and (2) Table II A shows that a higher percentage of girls than of boys scored 120 +, and this is in accord with the other data for the county area.

The somewhat lower scores in column (4) are doubtless due to the early transfer of some children to secondary schools, but on the whole there is close agreement for the brighter children (columns (3) and (4)).

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[1] To Mr W. J. Bright, Mr W. A. Chatters, Mr E. G. Davis, Mr S. H. Fitch, Mr A. L. Hurst, Mr H. H. Sterne and Mrs G. E. Sizer, I am specially indebted for the help they have given me during the period 1925–8.

Table I. *Percentages.*

	An Eastern County		Borough	
I.Q.'s	No. of children	%	No. of children	%
Above 140	7	0·2	4	0·95
131–140	37	1·3	3	0·71
121–130	99	3·3	24	5·7
111–120	301	10·2	51	12·12
101–110	560	18·9	97	23·04
91–100	793	26·8	126	29·91
81– 90	568	19·2	73	17·34
Below 81	593	20·1	43	10·2

Table II A. *County area.*

I.Q.'s expressed as percentages. Separation of the 10 and 11 age groups.

	10 years old			11 years old
	Boys (1)	Girls (2)	Boys and girls (3)	Boys and girls (4)
Above 140	0·4	0·4	0·4	0·1
131–140	1·2	1·0	1·1	1·3
121–130	2·5	4·4	3·3	3·3
111–120	9·1	12·4	10·6	9·7
101–110	18·4	22·7	20·5	17·4
91–100	24·3	27·6	25·9	27·7
81– 90	19·9	17·9	19·0	19·4
Below 81	24·2	13·6	18·2	21·1

Table II B. *Borough area.*

I.Q.'s expressed as percentages. Separation of the 10 and 11 age groups.

	10 years old			11 years old
	Boys	Girls	Boys and girls	Boys and girls
Above 140	1·8	1·7	1·9	—
131–140	0·9	—	0·5	0·9
121–130	9·0	6·8	7·9	3·0
111–120	15·3	11·3	13·2	11·1
101–110	23·5	19·9	21·6	24·9
91–100	20·7	32·8	26·8	33·6
81– 90	17·1	18·2	17·7	16·8
Below 81	11·7	9·3	10·5	9·7

In Table II B there is a marked difference between boys and girls aged 10—a difference that has been found in a subsequent investigation for a group 10–13 years of age.

The main differences between columns (3) and (4) are again due to the transfer of children to secondary schools.

III. FEATURES OF THE SURVEY.

The outstanding features of the county survey are that 4·8 per cent. of the children of 10 and 11 years of age scored I.Q.'s of 121 + and that 10·3 per cent. scored 115 +.

Prof. Thomson informs me that on the average he finds that 5 per cent.[1] of English children score 120 + and thus the figure 4·8 for this county does not differ significantly from his results in other areas.

On the other hand, there is a long tail, amounting to 20 per cent. with I.Q. scores below 81, and 28 per cent. of the departments produced no child with an I.Q. of 115 +.

There is a great difference between the county and the borough results, for in the borough 7·3 per cent. scored 121 + and 10·2 per cent. below 81.

Comparison with Northumberland.

On February 24th, 1922, all elementary school children in the county of Northumberland who were over 11 years and under 13 years of age worked a mental test and from data kindly communicated to me by G. H. Thomson it has been possible to estimate the I.Q. distribution of the Northumberland 11-year-old children for comparison with children of the same age in the present investigation.

Table III. *Percentages.*

	I.Q.							
	Above 140	131–140	121–130	111–120	101–110	91–100	81–90	Below 81
Northumberland	0·2	1·5	5·4	17·8	26·8	24·0	17·0	7·3
An Eastern county	0·1	1·3	3·3	9·7	17·4	27·7	19·4	21·1

The physical and occupational characteristics of the people are very different in the two counties, and Table III reveals a marked divergence in the mentality of the children.

Contrasting the two counties, it is to be observed that while the proportion of rural acreage in the two counties does not differ by more than 1 per cent., 62 per cent. of the population of my own county is rural compared with 14 per cent. of Northumberland.

The following figures, taken from the Registrar-General's Census, 1921, give the population per 1000 males, age 12 and over, engaged in the six industries employing the greatest number of men in the administrative counties.

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[1] He has slowly come to the conclusion that the percentage is rather more than 5.

An Eastern county	
1. Agricultural occupations ...	365
2. Commerce	63
3. Undefined workers (mainly labourers)...	44
4. Builders	43
5. Metal workers	43
6. Road transport workers ...	35

Northumberland	
1. Mining and quarrying ...	202
2. Metal workers	147
3. Transport	90
4. Commerce	65
5. Undefined workers (mainly labourers)...	64
6. Agricultural occupations ...	53

The urban area of my county occupies only 6 per cent. of the county and the largest town has a population of 15,937, situated in the centre of 600 square miles of country. The Registrar-General reports that the county "with its general population of 365 agricultural workers per 1000, and its rural population of 521, is the most exclusively agricultural county so far reported on."

In rural Northumberland, the largest group of men, 198 per 1000, is engaged in coal mining, and the population is four times as dense as in the rural parts of the Eastern county, but in the latter county there are three times as many agricultural labourers.

It is now possible to compare the abilities of the children whose fathers are engaged in the main occupational groups characteristic of Northumberland and my own county.

Duff and Thomson[1] have shown that the average I.Q.'s of the children of miners, quarrymen and metal workers were as follows:

Miners (colliery workers miscellaneous)	98·4
Quarrymen	98·6
Metal workers	100·9

The scores of 595 children whose fathers follow agricultural occupations are classified in Table IV.

For the purposes of this investigation, rural schools were chosen likely to have many children of farm workers. The agricultural occupations were recorded of the fathers of all children who took the mental test in 1925.

IV. THE ABILITIES OF CHILDREN WHOSE FATHERS FOLLOW AGRICULTURAL OCCUPATIONS.

The figures in column (3) give more accurate information than the lower limits indicated in column (2), for the Northumberland test, No. 1, is not designed to measure I.Q.'s below 80. The lower limits in column (2) are estimates only arrived at by an extension of the table given on p. 5 of Thomson's *Manual of Directions* and by a consideration of the class position of each child.

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[1] Duff and Thomson, this *Journal*, 1923, XIV (2), 194.

Table IV.

	(1) No. of children	(2) Range of I.Q.	(3) No. scoring I.Q. 80 –	(4) I.Q. 120 +	(5) Median	(6) I.Q. for small groups
Gardener	24	126–68	2	1	104	—
Foreman on farm	17	147–83	0	2	99	—
Skilled artizan*	146	149–64	21 (14)	17 (11)	98	—
Blacksmith	17	126–73	2	2	97	—
Farmer	43	132–68	3 (7)	2 (4)	91	—
Small-holder	25	121–67	7 (28)	1 (4)	91	—
Cowman	14	115–71	3	0	91	—
Horseman	72	121–64	15 (20)	1 (1)	90	—
Shepherd	9	103–67	3	0	90	—
Stockman	11	124–72	1	1	88	—
Farm labourer	187	120–60	60 (32)	2 (1)	87	—
Gamekeeper	10	122–69	4	1	82	—
Motor ploughman	4	—	—	—	—	113, 94, 90, 81
Dairyman	3	—	—	—	—	106, 102, 76
Woodman	6	—	—	—	—	122, 116, 108, 107, 93, 92
Malting labourer	7	—	—	—	—	122, 109, 107, 106, 99, 90, 87

The figures in brackets represent percentages.
* Included for comparison.

For Northumberland test, No. 1, Thomson[1] found a correlation of about 0·8 with Terman's Stanford Revision of the Binet Scale.

In an independent calculation undertaken to check the accuracy of the estimates mentioned above, the correlation worked out at 0·8 + for a complete group of 26 children, including some sub-normal children, in one school.

Other figures for sub-normal children are given in columns *A* and *B*, and indicate the degree of approximation for the lower limits in column (2) of Table IV.

	A Estimated as described	*B* Terman's Stanford Revision of the Binet Scale
A	77	74
B	64	62
C	61	54
D	65	68
E	66	71
F	67	73
G	71	74

Table IV shows that the median for farm labourers is rather lower than that for cowmen, horsemen or shepherds, the *élite* of the workers on farms. Of the children of farm labourers, 1 per cent. scored I.Q. 120 + and 32 per cent. scored less than I.Q. 80.

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[1] Duff and Thomson, this *Journal*, 1923, XIV (2), 194.

For agricultural workers of all classes in Northumberland, the average I.Q. worked out at 97·6—a figure considerably above the indications of Table IV. Moreover, the average I.Q.'s for the children of miners, quarrymen and metal workers are higher than for agricultural workers in either area. In this connection it is important to note that "Miners and agricultural labourers have approximately the same effective fertility, though they reach this result by different paths. The former have a high birth rate and a high child mortality and the latter a lower birth rate and a low child mortality[1]."

Thus the marked differences observable in Table III are not unassociated with the general characteristics of the population in the two counties. Moreover, the percentage of mentally defective children attending public elementary schools is more than twice as great in the Eastern county as in Northumberland[2].

Although the number of cases summarized in Table IV is small compared with the extensive research of Haggerty and Nash[3] in the United States of America and the still wider enquiry of Duff and Thomson in England, the degree of agreement, fortuitous though it may be, appears to be noteworthy.

Table V.

		Medians	
	Average I.Q. Duff and Thomson	Haggerty and Nash	This paper
Skilled artizans	102 (approx.)	98	98
Blacksmiths	101·4	95	97
Farmers	98·4	91	91
Unskilled labourers	94·3	89	87
	(unclassified)		(farm labourers)

A comparison of rural schools.

In a recent paper[1], D. Caradoc Jones and A. M. Carr-Saunders gave a useful bibliography of the work done on "The Relation between Intelligence and Social Status," and they refer specifically to investigators, who (*a*) classify children according as to whether they live in 'good' or 'bad' parts of the town, and (*b*) those which classify them according to the occupations of the fathers.

The borough results under (*a*) are in accord with previous work, and although marked differences in the general level of intelligence were known to exist in rural schools, the magnitude of the variations, in an area almost exclusively devoted to agriculture, is greater than anticipated.

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[1] Carr-Saunders and Caradoc Jones, *Social Structure of England and Wales*, Oxford 1927.

[2] Annual School Report, 1926.

[3] *Journal of Educational Psychology*, 1924, xv.

In Table VI schools of approximately the same size are compared, and in column (8) the distances from the nearest railway station (R.) and town (T.) are given.

Table VI.

(1) Index No.	(2) No. of children in school	(3) No. of children tested	(4) % of col. (3) scoring under 100 I.Q.	(5) % of col. (3) scoring over 100 I.Q.	(6) I.Q.'s 115 and over	(7) Average I.Q.	(8) Miles from nearest railway (R.) town (T.)	
Schools with less than 50 children:							R.	T.
138	46	11	90·9	9·1	Nil	86·4	3	7½
134	45	15	80·0	20·0	Nil	85·6	½	4
146	37	11	63·6	36·4	116, 116, 127, 133	99·3	4	8
112	32	13	53·8	46·2	Nil	93·1	4	4
151	39	11	27·3	72·7	117, 120, 120	105·1	8	7
72	33	11	27·3	72·7	116, 117	105·2	5	6
Schools with 50 to 100 children:								
62	84	26	84·6	15·4	115	86·7	4	4
114	80	21	80·9	19·1	Nil	82·6	4½	6
32	68	20	80·0	20·0	117	90·0	4	7
79	97	23	73·9	26·1	116, 116	90·0	5	7
111	91	23	65·2	34·8	116, 119, 147	88·0	2	2
70	84	21	52·4	47·6	115, 116, 120	99·0	4½	9
Schools with 100 to 150 children:								
126	104	31	80·6	19·4	115, 116	92·4	1½	5
149 (B.)	144	45	75·6	24·4	128, 133, 135	88·2	0	T.
8	109	38	52·6	47·4	117, 122, 124, 134	97·4	6	8½
26	134	33	42·4	57·6	115, 115, 115, 124, 130	102·3	0	5½
94	145	40	35·0	65·0	115, 115, 116, 116, 119, 119, 119, 120, 120, 124, 125, 125, 126, 132, 140	106·8	0	7
121 (B.)	126	43	30·2	69·8	115, 131	92·6	0	T.
Schools with over 150 children:								
71 (B.)	208	71	80·3	19·7	121	86·8	Town with railway station	
118 (M.)	338	125	71·2	28·8	115, 115, 115, 116, 116, 118, 120, 120, 122, 136, 142	90·7	,,	
71 (M.)	249	67	61·2	38·8	117, 118, 121, 123, 133	93·0	,	
148 (M.)	311	90	56·7	43·3	115, 116, 117, 118, 121, 122, 123, 125, 130, 138	98·1	,,	
121 (M.)	260	45	51·1	48·9	119, 119, 120, 120, 122, 123, 123, 126, 138, 140	102·7	,,	
109	154	48	20·8	79·2	115, 115, 116, 117, 118, 118, 120, 120, 127, 127, 129, 129, 132, 134	107·5	,,	

V. A COMPARISON OF TEACHERS' ESTIMATES WITH THE RESULT OF THE MENTAL TEST.

It has already been stated that before the day fixed for the mental test, teachers were asked to classify the children into five groups:

A. To mean exceptionally intelligent.
B. To mean a little above the average.
C. To mean average ability.
D. To mean a little below the average.
E. To mean exceptionally dull.

The massed results for the county area are given in Table VII.

Table VII. *Teachers' estimates expressed as percentages.*

Ages of children (boys and girls)	A	B	C	D	E
10	2·4	10·9	40·6	31·0	14·9
11	2·0	10·0	45·0	30·0	13·0
10 and 11	2·2	10·5	42·8	30·5	14·0
Total	12·7 Advanced		42·8 Normal	44·5 Retarded	

For comparison with the teachers' estimates, a table was constructed expressing the difference between the mental age as determined by the test, and the chronological age of each child.

Table VIII shows the percentage of normal children, *i.e.* children whose mental age is neither advanced nor retarded by 1 year; and the percentages of children whose mental age is advanced or retarded by 1 to 4 years. Children retarded by more than 4 years are included in the − 4 group.

Table VIII. *Mental test.*

Advancement and retardation expressed as percentages.

Ages of children (boys and girls)	Advanced					Retarded			
	+4 y.	+3 y.	+2 y.	+1 y.	Normal	−1 y.	−2 y.	−3 y.	−4 y.
10	0·3	1·6	4·5	11·4	43·0	18·4	10·7	9·4	0·4
11	0·9	1·2	4·3	10·6	39·4	17·9	13·7	6·9	4·8
10 and 11	0·6	1·4	4·4	11·0	41·2	18·1	12·2	8·1	2·6
Total	17·4 Advanced				41·2 Normal	41 Retarded			

The comparison may therefore be expressed thus:

	Mental test %	Teachers' estimates %
Children advanced	17·4	12·7
Normal children	41·2	42·8
Children retarded	41·0	44·5

For the normal children; the figures agree fairly well, but for the brighter children, there is a considerable discrepancy due to the fact that the method of calculating I.Q.'s makes a proper allowance for differences in age, while there is no such automatic check underlying the teachers' estimates. Thus, it not infrequently happened that younger children who scored as much as 120 + were marked C. For example, in the borough, where 31 children scored 120 +, 13 were marked C.

A scrutiny of the returns shows that the standard of individual teachers varies, and depends on the quality of the children in the particular school. In schools where the general level of ability is low, a child of barely average ability may be marked B or even A and *vice versa.*

R. E. Marsden in a personal note dealing with an investigation in the S.W. of England confirms this view. He says: "In these small schools, teachers know the relative capacities of the children in their own schools, but they are not so well aware of the quality of their children as compared with those in other schools. The teacher's average child varies with the quality of the children in the school."

A re-survey after two years.

Notwithstanding the difficulties indicated in the previous section, it appeared possible that interesting information might be obtained through the medium of a questionnaire addressed to teachers before these children ceased to attend school. By the courtesy of the editor of the local Teachers' News, information was asked for, and replies were received from 59 schools.

1st Questionnaire	Analysis of replies
1. No. of children who took the mental test in 1925?	1. 1588.
2. No. of children who scored 95–105?	2. 506.
3. Is your present estimate of the abilities of these children in general agreement with the result of the mental test?	3. Seven head teachers say they disagree with the general result of the test for the 95–105 group. On analysis of the return the total number of children concerned in the seven schools is eleven.
4. In how many cases do you disagree?	4. There is disagreement in 77 out of 506 cases, *i.e.* 15 per cent.
5. In how many instances has the mental test led you to revise your opinion of individual children?	5. The test led to a revised opinion in 58 cases, 3·6 per cent.
6. How many children who scored 115 and over have been given accelerated promotion?	6. 94 children.
7. Have you found that promotion in these cases was justified?	7. Yes, except in 4 cases.

A second and fuller questionnaire was sent to another group of schools and 88 replies were received.

2nd Questionnaire	Analysis of replies
1. No. of children who took the test in 1925?	1. 1701.
2. No. of children who scored 95–105 inclusive? Percentage?	2. In schools with 20 or more examinees, the percentage varies from 9·5 to 63.
3. Is your present estimate of the abilities of this 95–105 group in general agreement with the result of the mental test?	3. 90 per cent. of the head teachers agree with the general result. Nine head teachers are not in agreement concerning a total of 21 children.
4. In how many cases in this group (95–105) do you disagree?	4. There is disagreement in 54 out of 523 cases, 10 per cent.
5. Is your present estimate of the abilities of the children who scored less than 95, or over 105, in general agreement with the result of the mental test?	5. 88 per cent. of the head teachers agree with the general result. Eleven head teachers are not in agreement concerning a total of 30 children.
6. In how many of the last-mentioned cases do you disagree?	6. There is disagreement in 109 out of 1178 cases, 9 per cent.
7. In how many cases has the result of the test caused you to revise your estimate of the ability of the children?	7. In 47 cases, 4 per cent.
8. How many children who scored 115 and over have been given accelerated promotion?	8. 93.
9. Have you found the promotion in these cases justified?	9. Yes, except in 2 cases.

The most interesting fact that emerges from this enquiry is that of 187 children who scored I.Q. 115 + in 1925 and were given accelerated promotion in the elementary schools in which they remained, all but 6 are reported in 1927 to be doing well, and head teachers state that the accelerated promotion of these children was justified.

The remaining replies to the questionnaire based on marks or subjective estimates may be compared with the results of a public examination and estimates of a similar character.

The heads of five secondary schools in the same area were asked to state:

(*a*) The number of cases in which the result of the School Certificate Examination amounted to a complete surprise.

(*b*) Number of cases in which minor variations from the Secondary School estimates occurred.

For this examination, 59 candidates sat. Two cases were reported under (*a*) and six under (*b*), a variation of 13 per cent.

Relationship of the mental test to a standardized attainment examination.

Much work has been done on the relationship of Intelligence Tests to normal examinations, for example R. R. Dobson[1] has shown that the I.Q.'s of various groups of boys and girls at school correlate with school estimates from 0·62 to 0·78.

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[1] Dobson, this *Journal*, xv.

Again, Wood[1] found a correlation of 0·70 between college success and the Thorndike Intelligence Test.

It is now possible to compare the results of the mental test carried out in the Eastern County with the results of a standardized attainment examination in Arithmetic and English.

For this examination, Dr Ballard's standardized papers in Arithmetic (Problems) and English (Silent Reading)[2], were used and a table was constructed from which the marks scored could be read in terms of 'subject' ages for English and Arithmetic. By adopting the method of Educational Quotients[3], a satisfactory allowance for age was made and the results were calculated thus:

$$\text{Educational Quotient (E.Q.)} = \frac{\text{Educational age}}{\text{Chronological age}} \times 100,$$

where the term educational age stands for the mean of the 'subject' ages for the two papers of the examination.

The degree of correspondence between the results of the mental test and the results of the attainment examination for a group of children at a secondary school and for the children of 10 and 11 in attendance at elementary schools in two small towns, A and B, is indicated:

(*a*) by writing down, for the secondary school group, the orders of merit (*i.e.* the I.Q. and E.Q. orders) in the two examinations;

(*b*) by expressing the degree of correspondence in terms of coefficients of correlation calculated by using Spearman's formula for ranks with Pearson's correction.

Secondary school group.

Mental test (I.Q. order)	1	2	3	4	5	6	7	9	9	9	11	12½	12½	14½	14½	16
English and arithmetic (E.Q. order)	1	2	3	4	5½*	7½	10½	5½	7½	9	10½	15	16	12	13	14

* The usual practice has been adopted. The 'ties' for the 5th rank are given the rank 5½.

Here, the children, 1st, 2nd, 3rd and 4th, in the mental test were 1st, 2nd, 3rd and 4th respectively in the attainment examination and the 16th in the mental test was 14th in the attainment examination.

Children in towns A and B and the secondary school group. The majority of children at A and B were pupils who scored I.Q. 115 +. At A there were 12, and at B 17 children, who scored between 85–114 I.Q. and were either selected by their head teachers or desired by their parents to sit for the attainment examination.

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[1] Wood, *Measurement in Higher Education, Columbia University.*

[2] Dr P. H. Ballard, *The New Examiner*, 1923, pp. 163–99.

[3] In this connection, I am indebted to C. A. Richardson for helpful criticism.

Table IX.

	No. of pupils	Age* range	Range of I.Q.	Range of E.Q.	Coefficients of correlation	P.E.
Town A	41	10·3–12·1	85–142	70–155	0·81	+0·038
Town B	42	10·2–12·1	85–147	85–144	0·80	+0·038
Sec. school	16	10 0–12·5	99–144	83–147	—	—

* Years and months.

VI. THE ANCESTRY OF A SELECTED GROUP OF SUPERIOR CHILDREN.

Vocations of parents and grandparents.

By the researches of Duff and Thomson, Haggerty and Nash, and other investigators, it has been established that on the average children of 'lower status' do less well in intelligence tests than do children of 'higher status,' and it has also been shown that it is not possible to predict from occupation to I.Q. in individual cases.

The wider question how far does a person's ability tend to correspond with that of his immediate ancestors as measured by status brings in the mother and the grandparents.

In Table X the vocations of parents and grandparents are given for a selected group of children, all of whom scored 130 + in Northumberland test, No. 1.

The Roman figures refer as a rule to the classification of vocations prepared by Winifred Spielman and C. Burt[1]. Doubtful figures are enclosed in brackets.

Class I. Higher professional administrative work. Mental ratio over 150.

Class II. Lower professional, technical and executive work. Mental ratio 130–150.

Class III. Clerical and highly skilled work. Mental ratio 115–130.

Class IV. Skilled work. Mental ratio 100–115.

Class V. Semi-skilled repetition work. Mental ratio 85–100.

Class VI. Unskilled repetition work. Mental ratio 70–85.

Class VII. Casual labour. Mental ratio 50–70.

Class VIII. Institutional. Mental ratio under 50.

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[1] *A Study in Vocational Guidance*, Table IV, p. 16, Industrial Fatigue Research Board.

Table X.

						Father's side		Mother's side	
Ref. No.	Index No.	Sex	I.Q.	Father	Mother	Grandfather	Grandmother	Grandfather	Grandmother
C.	19	G.	153	Blacksmith IV	Teacher II	Blacksmith IV	—	Farmer IV	D.S. V or VI
B.	2	G.	153	Engineer's foreman III	Cook V	Bricklayer V	Dressmaker IV	Brickmaker V	Housekeeper
B.	1	B.	150	Jeweller's porter V	D.S. V or VI	Porter V	—	Shepherd VI	D.S. V or VI
C.	113	B.	144	Carpenter IV	D.S. V or VI	Carpenter IV	—	Dockmaster III	Home V or VI
C.	57	G.	143	Postman V	D.S. V or VI	Horseman VI	D.S. V or VI	Blacksmith IV	D.S. V or VI
C. (N.)	118	B.	142	Compositor III	Teacher (elem.) II	Jockey	D.S. V or VI	Turf correspondent III	D.S. V or VI
B.	3	B.	141	Soldier	D.S. V or VI	Drayman V	D.S. V or VI	Horsekeeper VI	D.S. V or VI
B.	3	B.	140	Postman V	Nursery governess III	Head postman III	Cook V	Accountant's clerk III	Court dressmaker III
C. (N.)	121	G.	140	Grocer's clerk IV	Parlour maid V	Varied	Dressmaker IV	Gardener	D.S. V or VI
C.	58	B.	139	Small-holder	D.S. V or VI	Farm labourer VI	D.S. V or VI	Engine driver IV	Factory hand VI
C. (S.)	148	B.	138	Sergeant (musketry instructor)	Silk warper V	Mat maker VI	D.S. V or VI	Mat maker VI	D.S. V or VI
C.	57	G.	137	Station agent (Canada) III	Teacher II	Factory manager II	Home	Police constable IV	D.S. V or VI
C.	65	B.	136	Stockman VI	D.S. V or VI	Farm labourer VI	D.S. V or VI	Gardener	Machinist, cloth factory V
C. (H.)	68	G.	136	Machinist V	Dressmaker IV	Farm worker VI	Housekeeper	Farm worker VI	Dairy maid
C. (N.)	119	B.	136	Carpenter IV	Home	Carpenter IV	—	Farmer V	—
C.	109	B.	134	Corn merchant III	Home	General shop-keeper III	—	Clerk III	—
C.	146	B.	133	Horseman VI	D.S. V or VI	Horseman VI	D.S. V or VI	Shepherd VI	D.S. V or VI
C.	100	G.	133	Groom VI	D.S. V or VI	Groom VI	Nurse III	Brickmaker V	Cook V
C. (H.)	71	G.	133	Engineer's labourer VI	Machinist V	Engineer's labourer VI	Machinist V	Weaver V	Factory finisher V
C. (N.)	106	B.	131	Mechanical engineer II	Teacher (elem.) II	Engineer II	Home	Sailor	Cook (V)
C.	106	G.	131	Drug worker VI	Cook V	Drug worker VI	Dressmaker IV	Weaver V	Weaver V

Note. D.S. = domestic servant. Column 1, Schools: B. = borough; C. (S.), C. (N.), C. (H.) = small towns; C. = village.

Ref. No.	Index No.	Sex	I.Q.	Father	Mother	Father's side		Mother's side	
						Grandfather	Grandmother	Grandfather	Grandmother
C.	106	B.	131	Malting labourer VI	Home	Labourer VI	Home	Labourer VI	Home
C. (N.)	119	G.	130	School teacher II	Home IV	Business IV	—	Engineer III	—
C.	26	B	130	Painter V	D S. V or VI	Farm worker VI	D.S. V or VI	Farm worker VI	D.S. V or VI
C. (S)	148	B.	130	Master baker (IV)	Shop assistant IV	Carpenter V	Housemaid V	Jeweller III	Ladies' hairdresser IV
B.	2	G.	130	Grocer III	Housekeeper III	Labourer VI	Maid V	Labourer VI	Dressmaker IV

Note. D.S.=domestic servant. Column 1, School: B.=borough, C. (S), C. (N.), C. (H.)=small towns; C.=village.

Two problems appeared worthy of investigation:

(1) Does information concerning the status of both parents and the grandparents afford evidence of greater prognostic value than data with respect to the father alone?

(2) In a rural area is there any relationship between the I.Q.'s scored by children of the selected group and the birthplaces of the immediate ancestors?

The results obtained from (2) led me to record the birthplaces of the parents of a group of children who scored I.Q.'s of less than 80.

(1) Looking at Table X, the general character of the parental and grandparental ancestry is sufficiently obvious. An inspection of the data in each horizontal line indicates that 12 children had at least one relative of superior status, but for convenience of description reference is made below to the classification previously described. Of the 156 ancestors, 25 only (16 per cent.) had occupations in Class III or higher.

Of the 26 children, 12 had a parent or grandparent in Group III or a higher group, and in 5 of the 12 cases three of the ancestors may be so classified.

On further analysis of this incomplete table the ancestors of superior status are distributed as follows:

3 cases	Mother	Father	1 grandparent
1 case	—	Father	2 grandparents
1 "	—	Father	1 grandparent
1 "	Mother	—	3 grandparents
1 "	Mother	Father	—
1 "	—	Father only	—
1 "	Mother only	—	—
3 cases	—	—	A grandparent only

Fourteen children had no ancestor in Groups I to III.

It seems, therefore, that the answer to the first question is that a record of the status of the ancestors is of greater prognostic value than that of the father alone, but in more than 50 per cent. of the cases no forecast from status to I.Q. is possible.

The tentative hypothesis advanced in the 'Discussion' may account for this circumstance in the selected group[1].

(2) The relationship of I.Q.'s to the birthplaces of the immediate ancestors. When parents or grandparents are born in the same village, is an environmental effect—arising from a restricted outlook and its influence on the children—to be noted?

The group chosen for investigation included all who were awarded free places in 1925. The birthplaces of some parents could not be obtained, but as it appeared desirable to make the list as long as possible three children who scored I.Q.'s of 115 +, and for whose parents birthplaces were known, were included.

Table XI shows the birthplaces of each parent and grandparent and the number of ancestors born in the same village or town.

Parents or grandparents born in widely different counties (at least 6 counties distant) are indicated in the table	W.D.C.
Parents or grandparents born in different counties	D.C.
Parents or grandparents born in the same county	S.C.
Parents or grandparents born in different villages of the same county	D.V.
Parents or grandparents born in the same village	S.V.

S.V. in the case of grandparents refers to them alone, but the data in column (7) supplements this information.

When the cases now given in Tables XI, XII and XIII were arranged in descending order of merit, and the list divided into two equal sections—(1) with higher I.Q.'s, and (2) with lower values—it became evident that the birthplaces of the ancestors of (1) were slightly more static than the birthplaces of (2), so that no advantage due to varied environment appeared.

It was then decided to dissect the table into three parts to separate the parents born (*a*) in villages, from parents born in (*b*) local towns, (*c*) distant towns. The discovery was then made that in all but two cases of group (*a*) Table XI the parents were born in different villages or counties, and when grandparents were included the ratio of S.V. in the first half of the table to S.V. in the second half was as 9 : 14.

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[1] Karl Pearson, "The Law of Ancestral Heredity," *Biometrika*, 1902–3, II, 211.

[2] Karl Pearson, "On the inheritance of the Mental and Moral Characters in Man," *Biometrika*, 1904, IV (2), 131.

Birthplaces of immediate ancestors.

Table XI. *Villages.*

(1)	(2)	(3)	(4) Father and mother					(5) Father's side (grandparents)					(6) Mother's side (grandparents)					(7) No. of ancestors born in one village
Ref. No.	Sex	I.Q.	W.D C.	D.C.	S.C.	D.V.	S.V.	W.D.C.	D C.	S.C.	D.V.	S.V.	W.D.C.	D.C.	S.C.	D.V.	S.V.	
3	G.	153	.	.	×	.	×	.	.	×	×	.	.	.	×	.	×	4
113	B.	144	×	.	.	.	.	.	.	×	.	×	.	.	×	.	×	3
57	G.	143	.	.	×	×	.	.	.	×	.	×	.	×	.	.	.	3
4	B.	141	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	2
58	B.	139	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	2
57	G.	137	.	.	×	×		.	.	×	.	×	.	×	.	.	.	3
65	B.	136	.	.	×	×	.	.	.	×	.	×	.	.	×	×	.	3
109	B.	134	.	×	.	.	.	.	.	.	.	.	.	.	.	.	.	.
146	B.	133	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	3
100	G.	133	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	2
106	G.	131	.	.	×	×	.	.	.	×	.	×	.	×	.	.	.	3
26	B.	130	.	.	×	×	.	.	×	.	.	.	.	.	×	×	.	2
119	G.	130	.	.	×	×		.	×	.	.	.	.	.	×	.	×	2
3	G.	130	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	2
89	B.	128	.	.	×	.	×	.	.	×	.	×	.	.	×	×	.	4
121	G.	126	.	×	.	.	.	.	.	×	×	.	.	.	×	×	.	2
17	G.	126	.	×	.	.	.	.	.	×	×	.	.	.	×	×	.	2
113	B.	124	.	.	×	×	.	.	.	×	.	×	.	×	.	.	.	2
36	B.	124	.	.	×	×	.	.	.	×	.	×	.	.	×	.	×	3
37	B.	122	.	.	×	×	.	.	.	×	?	.	.	.	×	.	×	3
67	B.	121	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	0
	B.	118	.	.	×	×	.	.	.	×	×	.	.	.	×	.	×	2
37	B.	117	.	×	.	.	.	.	×	.	.	.	.	.	×	.	×	3
	B.	115	.	.	×	×	.	.	.	×	×	.	.	.	×	.	×	4
34	B.	115	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	2
128	G.	113	.	.	×	×	.	.	.	×	.	×	.	.	×	×	.	3
39	G.	109	.	×	.	.	.	.	.	×	.	×	.	.	×	.	×	3
45	B.	108	.	.	×	×	.	.	.	×	.	×	.	.	×	.	×	3
156	B.	106	.	.	×	×	.	.	.	×	×	.	.	.	×	.	×	3
33	B.	104	.	.	×	×	.	.	.	×	×	.	.	.	×	.	×	3

Table XII. *Parents born in local towns or towns and villages.*

(1)	(2)	(3)	(4) Father and mother					(5) Father's side (grandparents)					(6) Mother's side (grandparents)					(7) No. of ancestors born in one village or town
Ref. No.	Sex	I Q.	W.D.C.	D.C.	S.C.	D.T.	S.T.	W.D.C.	D.C.	S.C.	D.T.	S.T.	W.D.C.	D.C.	S.C.	D.T.	S.T.	
3	B.	150	.	.	×	T.V.	.	.	.	×	.	×	.	×	.	.	.	2
118	B.	142	.	.	×	.	×	.	×	.	.	.	.	×	.	.	.	4
71	G.	133	.	.	×	.	×	.	.	×	.	×	.	.	×	.	×	6
148	B.	130	.	.	×	T.V.	.	.	×	.	.	.	.	.	×	.	×	3
148	B.	125	.	.	×	T.V.	.	.	.	×	.	×	.	.	×	T.V.	.	3
121	G.	120	.	×	.	.	.	.	×	.	.	.	.	×	.	.	.	3
149	G.	120	.	.	×	.	×	.	.	×	T.V	.	×	.	.	.	.	2
3	B.	107	.	.	×	T.T.	.	.	×	.	.	.	.	.	×	.	×	5
71	B.	106	.		×	T V.	.	.	.	×	.	×	.	.	×	V.V.	.	3

T. = Town. V. = Village.

Table XIII. *Parents born in distant towns.*

(1)	(2)	(3)	(4)					(5)					(6)					(7)
			Father and mother					Father's side (grandparents)					Mother's side (grandparents)					No. of ancestors born in one village or town
Ref. No.	Sex	I.Q.	W D.C.	D C.	S.C.	D.T.	S T.	W.D C.	D.C.	S.C.	D T.	S.T.	W.D.C	D C.	S.C.	D.T.	S.T.	
4	B.	140	.	×		.		.	.	×	V.V..		.	×	.	.	.	?
148	B.	138	.	.	×	.	×	.	.	×	.	×	.	.	×	.	×	?
109	B.	134	.	×	.	.	.	.	.	×	.	?	.	.	×		?	?
121	B.	131	.	×	.	.	.	×	.	.		.	×	.	.	.	.	?
100	G.	129	×	.	.	.	.	.	×	.	.	.	.	.	×	.	×	?
16	B.	129	×	.	.	.	.	.	.	×	L.	.	.	.	×	.	×	?
141	B.	127	.	.	×	L.	.	.	.	×	L.	.	×	L.	.	G.	.	?
43	B.	127	.	×		.		.	.	×	V.V.	.	.	.	×	L.	.	?
25	G.	125	×	.	.	.	.	×	.	.	.	.	.	.	×	V.V.	.	?
81	G.	123	×	.	.	.	.	.	.	×	×	.	.	.	×	.	V.V.	?
121	G.	120	×	.	.	.	.	.	×	.	.	.	×	.	.	.	.	?
121	G.	119	.	×	.	.	.	.	.	.	×	.	.	.	×	.	×	?
71	G.	119	×	.	.	.	.	.	×	.	.	.	×	.	.	.	.	?

V. = Village. L = London. G. = Glasgow

It became of great interest, therefore, to determine the birthplaces of the parents of a group of children who scored less than 80 I.Q. This information is given in Table XIV, columns (1) and (2), and in columns (3) and (4) the occupation of the fathers are noted.

Table XIV. *Relationship of* I.Q.*'s below* 80 *to birthplaces of parents and to occupations of the fathers.*

	(1)	(2)	(3)	(4)
Ref. No.	No. of I.Q.'s below 80 in 23 schools	No. of cases where father and mother were born in the same village	Father engaged in agriculture	Father engaged in other occupations
141	2	2	2	—
56	3	2	2	Platelayer
109	1	1	—	Bricklayer
20	3	1	1	Carter, shopkeeper
49	2	1	1	Electrician
167	2	—	1	Sweep
100	1	—	1	—
34	1	1	1	—
146	1	—	—	Coachman
17	3	1	3	—
16	3	1	—	Grave digger, bus driver, groom
45	3	1	—	Carpenter, carter, unknown
125	3	1	2	Brickmaker
57	2	1	1	Soldier
47	3	2	1	Bricklayer, warrener
81	4	—	3	Soldier
39	4	1	3	Newsagent
91	4	4	4	—
83	3	1	3	—
33	6	2	2	Labourer, painter, postman, bricklayer
25	2	—	1	Roadman
76	4	1	2	Carrier, labourer
106	3	1	1	Blacksmith, brickmaker
Total	63	25	35	
Percentage		39·6	55·5	

Comparison with a group of children scoring low I.Q.'*s.*

While the parents of children scoring high I.Q.'s were with two exceptions born in different villages, 39·6 per cent. of the fathers and mothers of children scoring low I.Q.'s were born in the same village and, moreover, this percentage is increased to 61·9 if the village radius is extended five miles.

Of the fathers of this group, 55·5 per cent. were engaged in agriculture and of the mothers a very large percentage were employed before marriage in domestic service.

Thus·	Domestic service	55	63
	Factory workers	5	
	Land workers	2	
	Clerk	1	

While no direct evidence is presented here of the prevalence of intermarriage in small rural communities, the data is in accord with the view often expressed by interested observers that intermarriage is one of the causes of deterioration in the offspring.

VII. Discussion.

It has been noted that in every class of the community there is an immense range of ability in the offspring.

In this enquiry the contrast is not between the children of professional men, enjoying every environmental advantage, and the children of labourers with an entirely different upbringing, for we have seen that not more than 16 per cent. of the ancestors of the selected group were persons belonging to the great middle class.

The superiority of approximately 50 per cent. of the selected group is associated with superiority in the ancestry as measured by status.

In the remaining cases, superiority of the offspring cannot be so explained, but given good original stock it does not follow that this trait will be reflected in a rise of status: particularly is this the case in a rural community where without financial resources little improvement in status was possible before 1902 and is still by no means sure. Hence the good stock that has not been affected by intermarriage remains and gives rise to a small percentage of notable children. In this connection we have seen that the children of superior ability under consideration are almost invariably the offspring of parents born in different villages while a significant percentage of mothers and fathers of children of inferior ability were born in one village. It follows that if the birth of parents in different counties or different villages is a favourable factor for the offspring and the birth of parents in the same village an unfavourable factor we should expect the birthplaces of ancestors of children scoring 104 to 126 (Table XI, second half) to be more static than the birthplaces of ancestors of children scoring 128–153 (Table XI, first half).

The figures are given in Table XV.

Table XV.

		First half of Table XI	Second half of Table XI
	Favourable factors.		
Father and mother	Widely different counties	1	0
	Different counties	1	4
	Different village	11	11
	Total	13	15
Grandparents	Widely different counties	—	—
	Different counties	5	2
	Different village	14	13
	Total	19	15
	Unfavourable factors.		
Father and mother	Same village	2	0
Grandparents	Same village	9	14
	Total	11	14

The differences to be observed are only slightly in favour of the 128–153 group but all the children under consideration are above normal. Had data been available for a complete age group (see Table XIV) the differences would undoubtedly have been marked.

General observations only can be made on the question of environment.

There is little difficulty in detecting children of superior ability soon after they begin attendance at school, so that if superior environmental conditions of any kind are factors of moment the period up to 5 years of age is one of active influence. For a large proportion of the cases considered in Table X superior environment of the kind associated with favourable economic status was not a factor. What, for instance, were the superior environmental conditions in the cases of Index Nos. 65 and 146?

Other environmental factors may, however, be of moment, and of these the following appear to be promising lines of enquiry[1].

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[1] Since writing this section an interesting paper by Mary M. Wentworth on "Individual differences in the intelligence of children" has come to my notice. *Harvard Studies in Education*, 1926, VII, 35.

What is the effect of the mother's temperament on the mental development of young children?

What is the effect of bilingualism in home and school?

Is the language of the home—or the absence of it—a factor of supreme importance in the mental development of the offspring during the period before attendance at school begins?

Are initial defects in speech correlated with mental retardation at the age of 6 or at the age of adolescence?

Acknowledgment of the help and co-operation of teachers has been made. To Dr P. H. Ballard for helpful comments in the early stages of this enquiry I offer my thanks.

To Prof. Godfrey H. Thomson I am much indebted not only for the data supplied with reference to a Northumberland Age Group but also for the interest he has taken in the subject of this paper.

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PUBLICATIONS RECENTLY RECEIVED

An Outline of Social Psychology. By J. R. KANTOR. Chicago: Follett Publishing Co. 1929. Pp. xiv + 420. $2.40.

It is a little difficult to see exactly what this book is intended to be. In scheme it is a comprehensive text book, meant to introduce the student to the whole range of its subject-matter. But the treatment is spread out and lacks conciseness, so that no critical points seem to stand up very clearly. It goes without saying that Prof. Kantor shows a wide knowledge of the work that has preceded his own. Often he gives just that kind of excellently selected list of further readings which can be made only by an author of wide knowledge and good judgment. Also he has tried to think out carefully all that he wishes to say. He gives, too, plenty of illustrations. Yet the impression remains that on the whole the work is on a rather ordinary level of generality. There is a fondness for heavy terminology.

The position is that social psychology is a science of cultural conduct, and cultural conduct is (*a*) stored by a set of people, and (*b*) determined by a set of people. Prof. Kantor reviews the various ways in which such conduct has been or may be viewed; discusses its expressions and their functions; considers the bearing of these expressions upon individual conduct, and how they enter into typical 'humanistic situations,' such as 'the scientific situation,' 'the political situation,' and so on. It all seems excellent as a scheme, and yet it all remains remote and somewhat blurred in the book. Perhaps he has tried to cover too much ground.

The History of Psychology. By W. B. PILLSBURY. New York: W. W. Norton Co. 1929. Pp. x + 326. $3.50.

On the whole this is a very good book for the student to read. It is rather of the nature of a catalogue of names with notes attached to each name. But the selection of names is good and so far as the notes are concerned with an exposition of doctrines they are as accurate as they could well be within the limits of space imposed. Prof. Pillsbury casts a very wide net, for he begins with *Early and Classical Greek* psychology and ends with McDougall, the *Gestalt* psychologists, and the school of Spranger, Binswanger and Ewald. Sometimes he is astonishingly inaccurate as to his facts. The worst example of this is probably the brief reference to Dr W. H. R. Rivers, who is said to have been "connected with St John's College, Cambridge, in various ranks until he became its head." It would have been extremely easy to find out what Rivers's position at St John's College actually was. However, criticism of this kind of book is undoubtedly extremely easy. Taking it all through, it is an interesting and useful volume.

Luc de Clapiers, Marquis de Vauvenargues. By MAY WALLAS. Cambridge University Press. 1928. Pp. viii + 308. 12*s.* 6*d.* net.

The first 150 pages of this book deal with Vauvenargues's life. The story is exceedingly well told and is uniformly interesting, though it is not exciting. The next 70 pages or so deal, mainly in a descriptive but to some extent in a critical manner, with those writings of Vauvenargues which have some psychological bearing. Chief of these was his championship of intuition and feeling as being of very great importance in setting the course and content of thinking. Herein, the author maintains, Vauvenargues was well ahead of his time. The final portions of the book are concerned chiefly with Vauvenargues's ethical views. In the end Miss Wallas sums up and maintains that her hero's "notes on the psychology of thinking have an especial interest in the present time," and must be of permanent value because they are based upon direct, "sensitive and vivid observation."

For the volume as a whole there can be nothing but praise: it is a sympathetic, attractive and accurate piece of work. That it is a little spun out, and that Vauvenargues still remains a distinctly minor character in the history of psychological development, are not the faults of the author.

Scientific Method: its Function in Research and in Education. By TRUMAN L. KELLEY. Columbus: The Ohio State University Press. 1929. Pp. 195. $2.50.

The book reports five rather slightly connected lectures given by Dr Kelley at the State University of Ohio. The lecture form is adhered to throughout. Consequently here and there the book seems unnecessarily diffuse, and the style is somewhat conversational. Nevertheless Dr Kelley has many interesting and important things to say, and his study is well worth the time it takes to read. The first lecture deals with broad differences between different fields of research and the bearing of these upon the methods of investigation that must be adopted. The second lecture, called The Role of Judgment in 'Objective Measurement,' is largely a consideration of the vices and virtues of the questionnaire, and on the whole concludes it to have more of the latter than of the former. The third lecture asks In what Units shall we measure Intelligence and Achievement? and the fourth follows this up by considering the significance of recent scientific development upon research in education and heredity. The last lecture is somewhat apart from the others and consists of a well illustrated study of the mental characteristics of men of science. Dr Kelley makes a valorous and interesting attempt to say what mental characteristics a great man of science must possess, and adds a further list of traits which he very often does possess. It is a book that presents much material for discussion and might well be a starting point for many important investigations.

Studies in the History of Statistical Method. By HELEN M. WALKER. Baltimore: The Williams and Wilkins Co. 1929. Pp. viii + 229.

The topics dealt with are: The Normal Curve, Moments, Percentiles, Correlation, The Theory of Two Factors, Statistics as a Subject of Instruction in American Universities. A very useful glossary of terms is added under the title: The Origin of Certain Technical Terms used in Statistics. An appendix deals with The Study of College Catalogues, and there is a capital bibliography. Several excellent portrait illustrations adorn the book. It will be seen that the book is rather a collection of studies of selected topics than a systematic history. Sometimes even there, as in The Theory of Two Factors, the treatment is no more than an annotated bibliography. As a most useful book of reference, however, the volume deserves to be widely known.

The Pedagogical Value of the True-False Examination. By A. W. COOKS. University Research Monographs, No. 7. Baltimore: Warwick and York. 1929. Pp. x + 115. $2.60.

As a result of his interesting experimental survey Mr Cooks concludes that the 'true-false test' is definitely superior to the essay type of examination. He gives his reason for this in detail, and discusses, always in the light of actual results, various means of making the test yield its most reliable and fruitful information. The monograph is of considerable interest and is very clearly written.

The Value of Homogeneous Grouping. By T. L. PURDOM. University Research Monographs, No. 1. Baltimore: Warwick and York. 1929. Pp. 99. $2.08.

The author carried out a comprehensive survey of the value of homogeneous grouping in five schools in Missouri, U.S.A. and here records his results in detail. On the whole the results do not show any marked advantages of the method of grouping in question. The work appears to have been carried out with care and thoroughness.

Individual Psychological Treatment. By E. WEXBERG, trans. by A. EILOART. London: The C. W. Daniel Co. 1929. Pp. 161. 6*s*. net.

The book gives a clear and readable account of the practical principles underlying the therapeutic work of Dr Alfred Adler. It is one of a series entitled *Psychic Methods of Cure*, and it is greatly to be hoped that the unprofessional reader who takes up one of these books will proceed to read them all. If he does not he may be led to attempt immediate application on the basis of very one-sided and inadequate knowledge. The danger of this is apparent. And when, as undoubtedly in the present case, the exposition is particularly clear and simple, it is a danger the results of which may be extremely serious. Subject to this caution, the book may safely be recommended as a good presentation of its subject-matter.

Problems in Psychopathology. By T. W. MITCHELL, M.D. London: Kegan Paul. 1927. Pp. v + 190. 9*s*. net.

This volume contains eight lectures given to members of the British Institute of Philosophical Studies. Beginning with an account of the 'pre-Freudian' school of Janet and Morton Prince, Dr Mitchell shows how the former's descriptive account of dissociation, as being due to some lack of psychic tension, is inadequate in that the mechanical effects of decrease of tension are given, but no explanations are offered of the dynamic causes of the decrease or of the reasons why hysteria may develop in one case and psychasthenia in another. These explanations were given by Freud, who introduced the concepts of Libido, repression and the unconscious. An account of his work takes up practically the whole of the rest of the book. The gradual development and elaboration of Freud's theories are traced from their beginnings in the treatment of hysteria, while the present day psycho-analytical views and problems are presented with a clarity and with a restraint which is not usual. The result is a volume which can be confidently recommended as an introduction to the large mass of psycho-analytical literature.

The 'post-Freudians' (as Dr Mitchell calls them) Rivers, Jung and Adler, receive scant attention; but, in the limits prescribed by the lectures, this was unavoidable.

Clinical and Abnormal Psychology. A Text book for Educators, Psychologists and Mental Hygiene Workers. By J. E. WALLIS WALLIN, Ph.D. Boston, New York, Chicago, Dallas, San Francisco: Houghton Mifflin Co. 1927. Pp. xxii + 650.

This volume has been specially prepared for those whose duties bring them into contact with educational clinics where individual children are examined. As such it deals primarily with the various forms of tests—intelligence, sensory, memory, motor ability, temperament etc.—and with the methods of carrying out a psychological examination, the difficulties of which are dealt with at some length. The whole is connected together by an outline of normal and of abnormal psychology.

The work is to be recommended. It is well balanced and the various views and theories are put forward and criticized with lucidity and restraint. Its value is considerably increased by the classified bibliography of over fourteen pages of titles.

Die Hauptrichtungen der gegenwärtigen Psychologie. Von R. MÜLLER-FREIENFELS. Leipzig: Quelle und Meyer. 1929. 145 S. M. 1.80.

This book should be useful to the student who, having already attained a fairly stable point of view, wishes to see whether or not there is any order in the apparent chaos of current psychological theories. After a general introduction, Prof. R. Muller-Freienfels divides current theories into two groups: those having a general objective trend, and those having a general subjective trend. In the first group are: association psychology, Wundtian psychology, theories having a general sensory motor basis, the views of E. Jaensch, the work of the Würtzburg school, Gestaltpsychologie, Behaviourism, and the study of conditioned reflexes, and a rather indefinite range of views called 'Parallelisten und Dualisten.' In the second group are: phenomenology, personality theories, 'Lebenspsychologie' (Avenarius, Groos, R. Muller-Freienfels), psychoanalysis, individual psychology (Adler), medical psychology (Binswanger, Kritchmer, etc.), characterology, and 'Die geisteswissenschaftliche Psychologie' (Dilthey, Spranger). All these are briefly characterized. References to appropriate literature are given, and in some cases critical studies are also indicated. Whether, having perused all this, the student will find his views clarified is more than doubtful: but at least he will have plenty to think about.

Grundfragen der Wahrnehmungslehre. Von P. F. LINKE. München: Ernst Reinhardt. 1929. xxvi + 430 S. M. 13.16.

This able and critical survey of general theories of perception is substantially a reprint of the first edition, but includes about fifty pages of 'concluding remarks.' This addition is concerned at length with developments of Husserl's phenomenology, rather briefly with the work of Joseph Geyser and some views of Driesch, and with Stumpf's *Empfindung und Vorstellung*, and again at length with recent contributions of *Gestalttheorie*. All these are considered in their general theoretical rather than in their experimental aspects. The volume remains one worthy of the most careful study of all students who are attracted by the epistemological aspects of perceptual problems.

Psychologie der frühen Kindheit. Von W. STERN. Leipzig: Quelle und Meyer. 1928. xv + 539 S. M. 15.80.

Prof. Stern's book is already well known to English readers from its translation. It has now reached a fifth edition and has been thoroughly revised, and is expanded in various parts. It contains also a section by Prof. Kurt Lewin dealing with the psychological significance of the movements of the young child. This is illustrated by reproductions from Prof. Lewin's cinematographic records. A further brief section by Prof. Heintz Werner deals with 'magical' reactions in the young child.

The book is a classic of observational and experimental studies of the more qualitative kind, and can be neglected by no student of the psychology of early childhood.

Freud's Tragischer Komplex: Eine Analyse der Psychoanalyse. Von CHARLES E. MAYLAN. München: Ernst Reinhardt. 1929. 215 S. M. 7.80.

The author cheerfully and at length analyses psychoanalysis. He builds upon early recollections and dreams of Freud, and finds that much can be explained in the light of Jewish tradition and belief. The writing is lively enough, but most readers may think the explanation somewhat forced. However it may be admitted that the author has succeeded in an attempt to provide an amusing entertainment, and a great many of his remarks are undoubtedly very shrewd.

Psychologie. Von H. K. SCHJELDERUP. Berlin und Leipzig: W. de Gruyter. 1928. xii + 330 S. M. 10.

This German translation of Prof. Schjelderup is the work of Max von Leinner. The text book itself follows traditional lines in the main. After a general introduction dealing with methods and historical development, the author states his general point of view. This is on the whole very much in line with modern functional and genetic psychology. The book then deals in turn with sensory responses, instinctive tendencies, temperament and intelligence, with the general principles of apprehension and learning, with memory reactions in most of their many forms, with personality and difficulties arising from its development. A short final section deals with modern applications of psychology. The treatment is judicious as well as comprehensive and shows a wide acquaintance with the best psychological work of several countries. As an introduction in general terms the volume is undoubtedly a good one.

Kindheit und Jugend: Genese des Bewusstseins. Von CHARLOTTE BÜHLER. Leipzig. 1928. xx + 307 S. R.M. 10– .

This is the third volume of the series of psychological monographs edited by Prof. Karl Bühler the general purpose of which is to build up the psychological history of the human individual from his birth onwards. It is a thoroughly interesting and workmanlike production, containing not a mere survey of the contributions of other writers but the record of a mass of important observational and experimental research carried out by the author and her assistants. The story proceeds from the first year to the period of adolescence. Successive chapters deal with the characteristic reactions of the first year, of years two to four, of years five to eight, of years nine to thirteen, and of years fourteen to nineteen. Naturally the most detailed studies are of the earliest years, but throughout the treatment is marked by a wide range, considerable sympathetic insight, and extremely clear exposition.

Contributi del Laboratorio di Psicologia e Biologia. Serie Terza: Pubblicazioni della Università Cattolica del Sacro Cuore. Milano. 1928. Pp. 436. L. 40.

This volume contains some unusually interesting records of research. First comes a long paper by Necchi Ludovico on *Ricerche medico-statistiche sui fanciulli anormali.* This is followed by two papers by Pastori Giuseppina dealing with medical and psychological problems connected with the functioning of the pineal gland. Castiglioni Giulio contributes a well documented study of the idea of God as he has found it in girls. An experimental study of eidetic imagery by G. M. Vacino, illustrated by a number of actual reproductions, follows. Gemelli Agostino contributes two papers, one a general introduction to the study of perceptual process, and the second an experimental investigation of the appearance and disappearance of perceived forms. Two studies by Gatti Alessandro are also included: an illusion in the field of kinaesthetic-tactual experience, and perception of wholes through the successive presentation of parts. Finally Larna Alberto writes on the tactual appreciation of form. All these papers are well presented, the whole work deals with genuine problems, and wherever it is experimental the methods are admirably adapted to the questions studied.

A Sceptical Examination of Contemporary British Philosophy. By ADRIAN COATES. London: Brentano's Ltd. 1929. Pp. 256. 10*s*.

A psychologist with a strong taste for general theory would find this book attractive. In ten short chapters ten contemporary philosophers are attacked or supported—mostly attacked. Mr Coates writes with energy and a refreshing lack of reserve.

Types of Philosophy. By W. E. HOCKING. London: Charles Scribner's Sons. 1929. Pp. xi + 462. 7*s.* 6*d.* net.

Prof. Hocking having very lucidly discussed a large variety of views put forward by different philosophers, at length confesses that he is a kind of idealist, and briefly explains what kind.

A Modern Theory of Ethics. By W. OLAF STAPLEDON. London: Methuen and Co. 1929. Pp. ix + 277. 8*s.* 6*d.* net.

The sub-title of this book is: A Study of the Relations of Ethics and Psychology. Dr Stapledon considers that we mean by 'ethically good' that which "is the fulfilment, or progressive fulfilling, of teleological tendencies objective to consciousness." This obviously lands him in an attempt to say definitely what the notion of tendency in modern psychological discussion really involves. His remarks under this head and in the ensuing discussion of 'psychical conflict' are well worth study, whether or not his application of them to ethical problems is accepted. Naturally they still leave a number of difficulties, as, for example, whether tendencies are in any sense structural. The attempt to clear up the expression "teleological tendencies objective to consciousness" is valiant, but unsatisfying.

SEVENTH INTERNATIONAL CONGRESS OF PHILOSOPHY.

(*First Circular.*)

IN accordance with the arrangement made by the Permanent International Committee in 1926, the SEVENTH INTERNATIONAL CONGRESS OF PHILOSOPHY will meet in September 1930 at Oxford.

The date of the Congress will be September 1st to 6th, 1930.

The recognized languages will be English, French, German and Italian.

The programme of the Congress will follow the lines of the Programme for the Sixth International Congress held in America in 1926. The Sessions will be arranged in four Divisions: A. Metaphysics; B. Logic and Epistemology; C. Ethics and Political Philosophy; D. History of Philosophy. Each Division will have one general meeting, and each Section a special meeting. The papers will be read by specially invited delegates from the various countries. The number of speakers to be fixed beforehand cannot exceed for a Divisional general meeting 4 or for a Sectional special meeting 3. The time allotted to papers will be 20 minutes each.

Membership in the Congress will include Active Members and Associate Members (the families of Active Members). The fee for Active Members will be £1 and for Associate Members 10*s*. Active Members will be entitled to a copy of the *Proceedings*.

All correspondence regarding the Congress should be addressed to Mr A. H. HANNAY, Secretary and Treasurer, 40 Well Walk, Hampstead, London, N.W. 3.

The detailed plans for the Congress and arrangements for accommodation will be announced later.

For the Organizing Committee.

J. A. SMITH, *Chairman.*

A. H. HANNAY, *Secretary.*

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

SECTIONAL MEETING.

Medical.

June 26th, 1929. "An Analysis of Psycho-sexual Divalence in Women." By Dr E. H. Connel.

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AT 30 SEPTEMBER 1929

Revenue Account for the year ended 30 *September* 1929

Year to 30 Sept. 1928 £ s. d.	EXPENDITURE	£ s. d.	£ s. d.
50 0 0	*To* Rent		50 0 0
59 6 6	Printing		79 6 6
24 14 1	Hire of Halls		37 15 9
	Office and General Expenses:		
	Secretarial Fees	200 0 0	
	Addressing	17 10 3	
	Stationery	14 10 8	
	Postage and Telephone...	38 11 0	
	Audit Fee	15 15 0	
	General Expenses ...	13 18 2	
299 16 4			300 5 1
435 1 8	Journals Publication Expenses		262 2 2
15 6 1	Teas		11 3 9
5 2 8	Scottish Branch Expenses		8 0 0
6 0 0	Midland Branch Expenses		2 12 6
4 3 0	Bank Charges		10 0
23 19 5	Committee for Research in Education ...		— — —
48 12 3	Library Depreciation		48 13 11
972 2 0			800 9 8
242 11 0	Balance, being Surplus of Income over Expenditure		337 15 2
£1214 13 0			£1138 4 10

INCOME	£ s. d.	£ s. d.	Year to 30 Sept. 1928 £ s. d.
By Subscriptions 1929	1210 3 0		
1928	7 17 0		
Arrears written off subsequently received, etc. ...	30 12 6		
	1248 12 6		
Less,			
Resignations and Removals	201 12 0		
		1047 0 6	1126 5 0
Midland Branch (3 Associate Members) ...		7 6	— — —
Interest on Deposit Account		2 8 10	— — —
Interest on Investments		88 8 0	88 8 0
		£1138 4 10	£1214 13 0

£ s. d.		£ s. d.	£ s. d.
	Sundry Creditors		
	Rent	12 10 0	
	Scottish Branch	8 0 0	
	Midland Branch	2 5 0	
12 10 0			22 15 0
60 14 0	*Subscriptions in Advance*		50 14 6
	Publications Reserve		
172 0 0	Balance as at 1 Oct. 1928		172 0 0
	Revenue Account		
	Balance at 1 Oct. 1928 ...	3086 4 11	
	Add, Surplus of year ...	337 15 2	
3086 4 11			3424 0 1
£3331 8 11			£3669 9 7

	£ s. d.	£ s. d.	£ s. d.
Balance at Barclays Bank			
Deposit Account	500 0 0		
Less,			
Current Account Overdrawn	64 14 11		
		435 5 1	79 10 10
Investments (at cost)			
£100 New South Wales 5½ % Loan 1924–34	100 0 0		
£1000 4 % War Loan 1929–42	1013 5 6		
£1100 5 % War Loan 1929–47	1117 6 9		
	2230 12 3		
Note. Market value of above securities at 30 Sept. 1929 £2207. 15*s.* 0*d.*			
500 National Savings Certs. (dated 20 Feb. 1923) ...	400 0 0		
		2630 12 3	2630 12 3
Subscriptions in arrear		165 6 6	183 15 6
Library			
Balance at 1 Oct. 1928 ...	437 10 4		
Expenditure during year ...	49 9 4		
	486 19 8		
Less, Depreciation	48 13 11		
		438 5 9	437 10 4
(Of the Expenditure on Library of £150 authorised on 8 March 1928 the sum of £110. 17*s.* 9*d.* has been spent to date leaving a balance of £39. 2*s.* 3*d.* unexpended.)			
		£3669 9 7	£3331 8 11

Note. Stock of Journals. There exists a number of past and present volumes of Journals in Stock, but owing to the possibility that a large proportion of these may never be sold, no valuation can be placed on such stock.

We beg to report that we have examined the foregoing accounts with the books and vouchers of the British Psychological Society and that we have obtained all the information and explanations we have required. In our opinion the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of our information and the explanations given to us and as shown by the books of the Society. Investments, which are lodged with the Bank, have been verified by production of Banker's Certificate.

A. J. HARPER & CO.
Incorporated Accountants.

35, Copthall Avenue, London, E.C. 2.

21 *November* 1929.

COMMITTEE FOR RESEARCH IN EDUCATION

ANNUAL REPORT.

THE Committee for Research in Education has met twice during the year 1928–9. It has been mainly concerned with the formulation of problems on which research in education is urgently needed and in collecting information with regard to the need of financial help for work that is in progress. Professor Burt and Mr Tibbey very kindly undertook to prepare a memorandum on the subject.

The following problems have been stressed as some on which research is specially needed:

1. The improvement and standardization of psychological and educational tests, and including not only tests of intelligence and of the ordinary Elementary curriculum, but also of special psychological capacities such as memory in its various forms, etc.

2. The validity of the Junior County Scholarship Examination as a means of selecting abler children; the investigation of supplementary methods, *e.g.* intelligence tests, teachers' judgments, etc.

3. The reliability and standardization of teachers' judgments generally, including judgments of temperamental and moral qualities as well as of intellectual and educational.

4. The characteristics of the supernormal child, family history, home circumstances, development, special capacities, temperament and character.

5. The handicaps of the child from the poor home.

6. The effect of absence from school on school attainments (it has been suggested that many children who do not begin reading or calculating until comparatively late in their school life nevertheless after a few years usually reach the attainments proper to their mental age. How far is this true?).

7. The causes and treatment of special disabilities in reading, spelling, and arithmetic.

8. Collating methods of treating dull and backward children.

9. The psychology of the partly deaf and the partly blind, with special reference to their education.

10. The effect on school progress of minor disabilities such as left-handedness, stammering, etc.

11. The relative value of special methods of teaching particular aspects of elementary subjects: *e.g.* in reading, the look-and-say method, the phonic method, etc., etc.; in arithmetic, the various methods of teaching subtraction, proportion, etc.

12. The nature, frequency, and treatment of neurotic children.

13. The change in character-qualities at 11. (The clean-cut implies that after the age of 11 children require a different outlook, and that this need is dependent on chronological or physiological age rather than upon mental age. This is an assumption that needs to be checked. One approach might be to examine the different character-qualities of individuals having the mental age of 11, whether those individuals were borderline defectives of 15 or 16 or supernormal children of 8 or 9.)

It has been found that work on the following topics is in progress and that the workers would welcome financial support for the continuance of the work.

1. Mental Characteristics of Physically Defective Children.
2. Young Children's Language.
3. Plateaux in Learning.
4. The Effect of Incentives on Learning.
5. Modern Studies in Temperament and their Educational Significance.
6. Educational Problems connected with Bilingualism.

Inquiries were sent to the training colleges with regard to the syllabuses drafted by a joint committee of some members of this committee and of the Training College Association. The replies showed that the syllabus is regarded as too difficult and too comprehensive for use by lecturers in two-year elementary training colleges.

Twelve inquiries with reference to tests and methods have been answered, and the summary of research in progress appeared in the April number of the *Journal*.

V. HAZLITT.
Hon. Secretary.

FINANCIAL STATEMENT FOR YEAR ENDING, 31ST OCTOBER 1929.

RECEIPTS.	£	s.	d.	EXPENDITURE.	£	s.	d.
By Balance	14	10	8	*To* Typing and duplicating	3	1	9
,, Special receipt		6	2	,, Postages	2	13	6
				,, Printing	1	0	0
				,, Hire of Rooms	1	0	0
				,, Balance	7	1	7
	£14	16	10		£14	16	10

Audited and found correct.
T. G. TIBBEY.
26 *November* 1929.

Signed, A. E. TWENTYMAN,
Treasurer.

VOLUME XX APRIL, 1930 PART 4

THE TECHNIQUE OF EXPERIMENTATION ON THE PSYCHO-GALVANIC REFLEX PHENOMENON AND THE PHENOMENON OF TARCHANOFF. II.[1]

BY ROBERT H. THOULESS.

(From the Department of Psychology, the University, Glasgow.)

III. *The measurement of the T-phenomenon* (pp. 309–313).
IV. *Apparatus* (pp. 313–317).
V. *Various factors determining* W' (pp. 317–319).
VI. *Preliminary problems* (pp. 320–321).

III. THE MEASUREMENT OF THE T-PHENOMENON.

THE essential problem in devising a satisfactory circuit for the measurement of the T-phenomenon is to measure the change in somatic E.M.F. in such a way that the result obtained will not be affected by the simultaneous polarizability change (or resistance change) of the P.G.R. For the solution of this problem (just as for the problem of devising a satisfactory circuit for the P.G.R.) it makes not an atom of difference whether the P.G.R. is a resistance or a polarization change. If it is a polarization change, the somatic current produces its own back E.M.F. of polarization just as does a current from an external source, so a decrease of the subject's polarizability causes an increase in the somatic current indistinguishable from that caused by an increase of the somatic E.M.F. How is the true increase of somatic E.M.F. to be separated from this effect?

There are two methods of reducing the effect of the P.G.R. to a minimum while still recording the T-phenomenon. Tarchanoff's method (7) is by the use of the circuit shown in Fig. 5 *A*. The current change in the P.G.R. is proportional to the total current through the body. If the total current is made small by neutralization of the somatic E.M.F. (ϵ) by means of an external E.M.F. (E), the P.G.R. effect will be reduced while the T-phenomenon will remain unchanged. An external E.M.F. is introduced into the circuit by means of a potentiometer in an opposite direction to the somatic E.M.F. until the resultant current through the body is zero (so $E = -\epsilon$). Fig. 2 *A* (2) shows a T-phenomenon obtained by the method of Tarchanoff. The P.G.R. is not entirely eliminated by this method, since there is a deflection at the end of the reaction and this deflection has been increased by the P.G.R. by a fraction of its total amount equal to $\Delta W'/W'$—*i.e.* with an average P.G.R. the error will be of the order of 10 per cent. The error would be zero only if the deflection were zero at the end of the reaction, a condition impossible to secure in practice. This method has the advantage that the current changes are not too small for convenient measurement with a reasonably sensitive galvanometer, but has the disadvantage of requiring repeated adjustments of the potentiometer.

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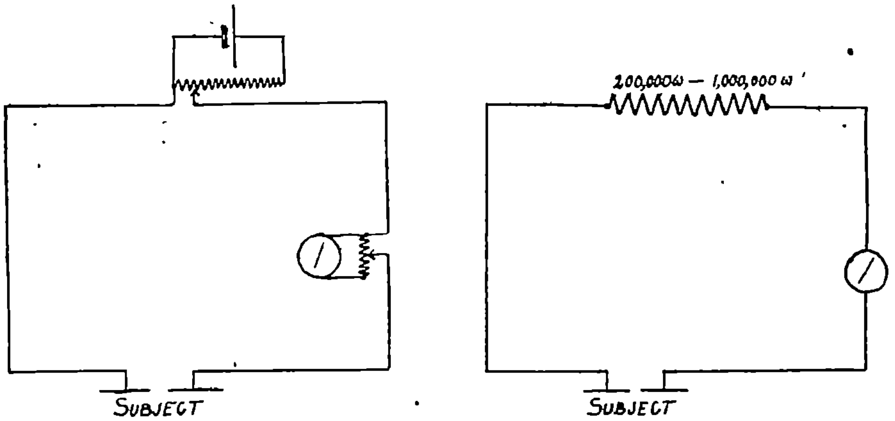
[1] The first part of this paper appeared in this *Journal*, Vol. XX, 3, 1930.

An alternative method which I have generally used is shown in Fig. 5 *B*. No external current is used but a large resistance is placed in series with the subject. This resistance must be made so large that the change in W' in the P.G.R. is negligible in comparison with it. The galvanometer must be sensitive enough to give a reasonable deflection for the very small current which will flow through such a circuit. Its change of deflection will then be a measure of $\Delta\epsilon$ with negligible influence of $\Delta W'$. This method has the advantage of eliminating the need for any adjustments during the course of the experiment. It has, however, several disadvantages which are the result of the extreme smallness of the currents used (the top record in Fig. 2 *C* records a current of $1{\cdot}2 \times 10^{-8}$ amps., while the current change is only $1{\cdot}5 \times 10^{-9}$ amps.). First, even sensitive galvanometers give only small deflections for currents of this order, so high precision of measurement is impossible. Secondly, scrupulous care must be taken in the insulation of the whole circuit since



A. Tarchanoff's circuit.   B. Alternative circuit for T-phenomenon.

Fig. 5.

stray currents of this order are possible. Thirdly, the external resistance used is of the same order as the resistance of the insulation in some electrical apparatus (for example, on switches with a slate base), so bad mistakes in the calculation of results may be caused by the presence of such insulation in the circuit.

We may calculate the order of error produced in these different methods by using the values of the P.G.R. and T-phenomenon shown in Fig. 2. If we have a circuit with external E.M.F. $= E$ and external resistance $= R$, then i_1 (current before the reaction) $= \dfrac{E + \epsilon - p}{W + R}$ and i_2 (current after the reaction) $= \dfrac{E + \epsilon + \Delta\epsilon - (p - \Delta p)}{W + R}$. Since, for small values of i, p will be proportional to the current, we can without error throw these equations into a form more convenient for our present purpose by treating the changes in p as changes in the subject's "apparent resistance." Then

$$i_1 = \frac{E + \epsilon}{W' + R} \quad \text{and} \quad i_2 = \frac{E + \epsilon + \Delta\epsilon}{R + W' - \Delta W'}$$

and

$$i_2 - i_1 = \frac{(R + W')\,\Delta\epsilon + \Delta W'\,(E + \epsilon)}{(R + W')\,(R + W' - \Delta W')}.$$

The condition for the satisfactory measurement of the T-phenomenon is that $i_2 - i_1$ should be determined mainly by $\Delta\epsilon$ and only to a negligible extent by $\Delta W'$. In Tarchanoff's method $R = 0$ and $E = -\epsilon$, so $i_2 - i_1$ reduces to the form $\Delta\epsilon/(W' - \Delta W')$, in which W' has disappeared from the numerator but remains in the denominator[1], causing an increase of the T-phenomenon deflection by a fraction of its total amount,

$$\Delta W'/(W' - \Delta W'),$$

or approximately by $\Delta W'/W'$. In the curves shown in Fig. 2, $\Delta W'/W'$ was about 0·115. Fig. 2 *A* (2), therefore, which shows a change in deflection of 5·1 mm., must (if we ignore the slight additional error due to the fact that the initial deflection was not quite zero) be regarded as made up of about 4·5 mm. T-phenomenon and 0·6 mm. P.G.R. An error of over 10 per cent. is thus made in the measurement of the T-phenomenon by this method, due to incomplete elimination of the P.G.R. To this objection must be added some measure of uncertainty in the expression of $\Delta\epsilon$ as a voltage change, since its value as a voltage change depends on the value of W' which cannot be measured with great precision by this method.

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[1] W' would only disappear altogether from the formula if E were made equal to $\epsilon + \Delta\epsilon$ (*i.e.* on the condition already mentioned, that the deflection was zero after the reaction).

If R were made sufficiently large for $\Delta W'$ to be negligible, the expression for $i_2 - i_1$ would be reduced to $\Delta\epsilon/(R + W')$. Again we can discover from the curves in Fig. 2 how large R should have been for this condition to be approximately fulfilled. If E is zero (method of Fig. 5 B), $i_2 - i_1$ will be $\frac{(R + W')\,\Delta\epsilon + \Delta W'.\epsilon}{(R + W')\,(R + W' - \Delta W')}$. In this formula the influence of W' will be much larger in the numerator than in the denominator, so, as a close approximation, we may calculate the conditions under which $\Delta W'.\epsilon$ will be negligible in comparison with $(R + W')\,\Delta\epsilon$. The percentage error in neglecting $\Delta W'.\,\epsilon$ will be $\frac{100.\Delta W'.\epsilon}{(R + W')\,\Delta\epsilon}$. In Fig. 2 C, ϵ was found to be 0·026 volt, while $\Delta\epsilon$ was 0·003 volt. Taking $\Delta W'$ as $0{\cdot}115 \times W'$, this makes the percentage error equal to $\frac{100 \times 0{\cdot}115 \times 8{\cdot}7 \times W'}{R + W'}$ or very nearly $\frac{100 \times W'}{R + W'}$. Thus the error by this method is the same as that by the method of Tarchanoff if R is only $8\,W'$. If R is made greater, the error is less. The current becomes inconveniently small if R is made much greater than 200,000 ohms (the value it had for the recording of the curves in Fig. 2 C). This for $W' = 17{,}000$ ohms gives an error of 7 per cent. For many practical purposes this may be near enough, but it is not very satisfactory. A safe rule for this method would be that the external resistance must be not less than 20 times W' (giving an error of about 5 per cent.).

A much closer approximation to the pure T-phenomenon can, however, be got by combining the two methods. If E were made equal to ϵ, and an external resistance were used, the percentage error due to the P.G.R. would be $100\Delta W'/(R + W')$. In the examples taken this would be reduced to 2 per cent. if R were 90,000 ohms. For a general safe value of R, we may make the rule that if an external E.M.F. is used to neutralize the somatic E.M.F., there should also be an external resistance in the circuit of not less than ten times the subject's apparent resistance. This will reduce the error in measurement of the T-phenomenon through influence of the P.G.R. to 2 per cent. with a P.G.R. of $\Delta W'/W' = 11$ per cent. If large electrodes are used (of 20 cm.^{2} or over) W' will be small enough for such an R to leave the currents not too much reduced for accurate measurement with a sensitive galvanometer.

The one circuit for the measurement of the T-phenomenon which is quite indefensible for scientific purposes although it has been commonly used is one in which neither an external resistance nor a neutralizing external E.M.F. is employed, but the somatic current is passed directly through a galvanometer[1]. This simply gives a combined record of the P.G.R. and T-phenomenon with no possibility of separating the one from the other.

Some investigators have made an even more objectionable modification of this method by using electrodes of different materials (*e.g.* carbon and zinc). This is, in effect, to introduce into the circuit a very inefficient zinc-carbon cell producing an E.M.F. of small but unknown amount which will certainly change during the course of the experiment. If a small external E.M.F. is required it can be obtained in known and constant amount from an external cell with a potential divider. In no case should electrodes be used which are themselves possible sources of current.

IV. APPARATUS.

(i) *Galvanometers.*

For rough diagnostic experiments in electrical responses, any galvanometer can be used which gives sufficiently large deflections for the purpose in hand. Many of the unsolved problems in this subject will no doubt be solved by skilled observers using cheap long-period galvanometers. For many such purposes, no advantage would follow from the use of a more delicate and expensive instrument. There is, however, a large class of problems for which the ordinary mirror galvanometer is useless. Its period is commonly of the order of 7 to 8 seconds, a time during which no less than three changes in current take place in the complex form of the T-phenomenon. For problems involving the measurement of latent periods, the determination of the form of the photographed curves of deflection, or even the absolute height of deflection changes, a galvanometer of much smaller period is essential. The only alternative is to obtain photographic records and to correct observed deflections for the inertia of the galvanometer by the use of formulae (1), a practicable method if few observations are made but taking up far too much time if applied to the many observations necessary in any psychological investigation.

The requirements for a satisfactory galvanometer for both kinds of electrical measurement are:

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[1] Indefensible for scientific purposes, this circuit has many advantages for the popular demonstration of electric responses to a non-scientific audience. The deflections are large and (unlike the Wheatstone bridge circuit) this arrangement does not necessitate continual re-adjustments to keep the spot of light on the scale. The use of the circuit should, however, be strictly confined to such laboratory Punch and Judy shows.

(1) High sensitivity (for accurate measurement of the T-phenomenon).
(2) Short period (for the purposes mentioned above).
(3) A dead-beat deflection.
(4) Insensitivity to outside vibration (for photographic recording).
(5) A reasonable tolerance of accidental large currents (since the instrument will sometimes be handled by students who are inexpert in physical measurement).

The first two of these requirements are very much the most important. The last two are non-essential respects in which different makes of galvanometer differ considerably. The third requirement can be secured in any galvanometer (not wound ballistically) by choice of a suitable shorting resistance additional to the universal shunt used for adjusting the current through the galvanometer (or, in an instrument with an electromagnet, by controlling the strength of the magnetic field). The total resistance required to make any particular galvanometer dead-beat is generally stated by the makers. Otherwise, it can easily be discovered empirically. If the shorting resistance is too large, the galvanometer will swing past its point of deflection; if it is too small, the galvanometer will be too sluggish in reaching its point of balance.

The essential difficulty in the selection of a suitable galvanometer for all purposes is that the requirements (1) and (2) are antagonistic. I know of no galvanometer with negligible period which is sensitive enough to give satisfactory readings of the T-phenomenon. Many different kinds of galvanometer can be used, all with some disadvantages.

(*a*) *The ordinary cheap mirror galvanometer* (*d'Arsonval, Ayrton-Mather, and other patterns*). The kind of sensitivity required for the measurement of the T-phenomenon in a satisfactory circuit may be taken as 200 mm. per μ amp. at 1 metre. This is a minimum requirement; a higher sensitivity is better. Any instrument maker's catalogue shows galvanometers at from £3 to £4, of sensitivity greater than this. The periods of these instruments are, however, too long, ranging from 5 to 8 sec.

If, on the other hand, we are content with a rapid galvanometer of relatively low sensitivity (which is all that is required for the P.G.R.) there are two choices.

(*b*) *The string galvanometer*. This has been largely used for such purposes as the accurate determination of latent periods. The glass string commercially obtainable has a period of only 1/200 sec., while Prof. Einthoven himself worked with one with a period as short as 1/1000 sec. As an instrument for measuring deflections it is less suitable. Its

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deflections are not proportional to the current flowing through and bear no simple relation to it. This necessitates detailed calibration over the whole range of deflections to be used. It can be made sufficiently sensitive to give good deflections for the T-phenomenon only when this is in a low resistance circuit. For this purpose the string must be made very slack. Its sensitivity then shows continual large change all the time it is being used, so detailed calibration must be repeated for every separate observation.

The expense of the string galvanometer makes it impossible of acquirement by most psychological laboratories. There is, however, a cheaper instrument which is just as satisfactory that should find a place in every psychological laboratory in which accurate research on the time relationships of these phenomena is required.

(c) *The Kipp galvanometer.* This is an instrument with a very small mirror and a single loop of wire in a magnetic field. Its period is 1/50 sec. Although longer than that of the glass string, this is no disadvantage for any purpose for which it could be used in psychological investigation. It is convenient to use since its deflections (if small) are proportional to the current, and it works in any position. Its sensitivity is not great enough for T-phenomenon readings except in the unsatisfactory low resistance circuits. A further disadvantage is the ease with which it can be damaged by excessive currents.

(d) *The Moll galvanometer* is probably the best single compromise between the requirements of short period and high sensitivity[1]. Its sensitivity is just great enough for taking satisfactory readings of the T-phenomenon in high resistance circuits, while its period is about 1·4 sec. This period is short enough for most practical purposes, although not for very accurate determination of latent periods and maximum and minimum points. The Moll is very tolerant of excessive currents, but has the disadvantage of responding too easily to external vibration.

(ii) *Electrodes.*

Satisfactory electrodes should fulfil the following conditions:

(1) They should not themselves be possible sources of current. This is secured by making them of the same material. A further refinement, if they are of metal, is to cut both from the same strip. Soldered junctions must not be within reach of the electrolyte and are best kept quite away from the surface of contact.

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[1] The photographic records shown in Fig. 2 were made with a Moll galvanometer for which I am indebted to a grant made by the Industrial Fatigue Research Board.

(2) They should not produce a back E.M.F. of polarization when a small continuous direct current is passed through them. Such a back E.M.F. will be produced if the passage of current causes either a change in the chemical nature of the surface in contact with the skin or in the concentration of the electrolyte. Plates of zinc (as of any other metal) whether dry or moistened with a solution of common salt would be objectionable for the first of the above reasons; plates of zinc in bags moistened with zinc chloride solution would be objectionable for the second reason.

(3) They should form sufficiently good contact with the skin for this contact to be undisturbed by movement.

(4) At the same time, they must be comfortable and not fastened so tightly as to restrict the blood supply.

(5) There should be no possibility of progressive drying of the electrolyte or similar change.

These points are of very unequal importance. No. 3 is clearly most important for our purpose since this alone is a way in which unsatisfactory electrodes might lead to current changes simulating the reaction studied. Polarizability in the electrodes, on the other hand, could at worst lead to partial obscuring of the phenomenon and an artificial increase in the value of W'. These are to be avoided as far as possible, but scrupulosity about the non-polarizability of electrodes does not compensate for errors in other parts of experimental technique.

It is generally stated that no electrode is perfectly non-polarizable. Liquid electrodes can probably be made most nearly so. A simple non-polarizable liquid electrode is described in a recent number of *The American Journal of Psychology* (12).

The electrodes which I have used are similar to a pattern described by Dresbach (13). From the same strip of thin silver foil (of about 0·05 mm. thickness) two pieces are cut out. Each consists of a circular area of about 20 cm.^{2}, with an extension in the form of a straight strip about 1 cm. wide and 4 cm. long. Copper wires are soldered or otherwise attached to the ends of these strips. The electrodes are then coated with silver chloride by electrolysis in a sodium chloride solution. For attachment to the subject one surface of the electrode is coated with a thin film of gelatine moistened with Ringer solution. As an additional precaution against polarization, I use for this coating an emulsion of silver chloride in gelatine, but I do not know whether this is necessary or useful. The large size of the electrodes keeps the total resistance low and prevents evaporation. The electrodes are bound on with a bandage liberally padded with cotton wool; a woollen glove can be used in addition or can replace the bandage if there is sufficient padding. The guarantee of efficient contact is that on removing the bandages at the end of an experiment, the electrodes will be found stuck to the skin over their whole area, and, when they are peeled off, they are found to carry over their whole surface a sharp impression of the lines, hairs, etc., on the skin.

V. VARIOUS FACTORS DETERMINING W'.

It is well known to all experimenters that on first attaching electrodes to a subject he generally shows at first a very high resistance, differing largely from day to day, which drops during a period of from twenty minutes to half an hour. This change is probably the resultant of many different factors, its speed depends on the nature of the electrolyte and we may safely guess that penetration of the high resistance layer of the skin by the electrolyte is the predominant factor. This must, however, be complicated by other physical changes taking place at the electrodes (the rise of temperature at the skin surface, for example, which we know is not the predominant cause of the phenomenon since the initial fall of resistance is also found when the electrodes are attached to a corpse).

The interest of the absolute value of W' to the psychologist is that it is (other things being equal) an indication of the mental condition of the subject. I have expressed this mental condition as 'alertness' or the readiness to react to a stimulus (10); Mr Cattell considers that W' measures the fraction:

$$\frac{\text{Available instinctive energy}}{\text{Extent of release of energy taking place in consciousness}}\ (14).$$

The difficulty in making any use of measures of W' is the number of non-psychological factors which also influence its value, some of which only are controllable. The attempt to understand these and to control them as far as possible is necessary also in measurements of $\Delta W'$, since W' is the background against which P.G.R. changes occur and we are by no means certain that we can make significant comparisons of P.G.R. changes which occur with different values of W'.

(1) The first of these factors is the whole set of physical changes which determine the physical part of the initial fall of resistance. These changes cannot be avoided but can best be dealt with by putting off measurements until at least twenty minutes (better until half an hour) after the electrodes have been attached. Early investigators often measured the P.G.R. against a background of falling apparent resistance soon after electrodes were attached. There is nothing to be said for this practice and

it is now generally given up. Concentration of electrolyte will of course be carefully controlled, and to prevent osmosis from bodily cells it seems better to use a solution isotonic with the body fluids rather than 5 per cent. or 10 per cent. salt solutions which would tend to dehydrate the skin cells thus possibly altering their resistance.

(2) If half an hour after electrodes have been attached a subject walks about, his resistance will be found when he sits down again to be lowered and to be rising. This effect of general movement is another reason for making no observations for some time after the experiment has started. It will be noticed that the effect of this factor is in the opposite direction to that of the last one mentioned. It is possible that the discrepant results obtained in different observations of the initial change in resistance (which sometimes is a rise instead of a fall) are due to the occasional preponderance of this factor over the last, since at the beginning of an experiment the subject has probably been moving about.

(3) When a current starts flowing through the skin there is at the moment of contact no polarization, so the apparent resistance rapidly increases from a low to a high value as the polarization reaches its full value. This we may call the 'primary polarization change.' Experiment with string galvanometers shows that the greater part of the polarization is attained in a fraction of a second and that it is virtually complete in a few seconds. It is, therefore, too quick to be any trouble in practice. We are unlikely to try to measure W' in less than four or five seconds after completion of the circuit.

(4) There is, however, a slower change (which we may call the 'secondary polarization change') which sometimes requires consideration. The passage of a continuous current causes a decrease of polarizability[1]. Thus when a current begins to flow in the body, or when a continuous current is considerably increased, there is first a large current, decreasing for a second or two as a result of the primary polarization change, then the decrease changes into a slow increase as a result of the secondary polarization change. This change is slower. Three minutes is probably a safe time to allow for equilibrium after a considerable change in current has been made, but one minute is ample for most practical purposes. The P.G.R. should not normally be measured in much less than a minute after a considerable current change.

(5) Part of the secondary polarization effect is temporary, the change being reversed after the current has ceased. Part, however, is permanent,

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[1] Einthoven considers that this is a change in true resistance caused by the passage of a current. I have evidence against this view, but in practice the difference does not matter.

and this part calls for an additional control. If a very large current is passed through the skin, its apparent resistance to a small current is found to be very greatly reduced and to remain at a lower level. For example, after electrodes had been attached for half an hour, I measured the apparent resistance of a subject with an E.M.F. of 2·1 volts and found it to be 43,800 ohms. An E.M.F. of 22·8 volts was then applied for one minute, at the end of which *W′* as measured by that voltage was 2280 ohms. It was then measured again at the lower voltage and was found to be 3805 ohms and remained at that sort of level. This observation indicates that, in order to get significant values of *W′*, it is necessary to control not only the E.M.F. at the time of observation but also previous E.M.F.'s applied.

(6) The secondary polarization effect has a curious influence on *W′* when measured by electrodes on the back and palm of the hand. Change of the direction of the current causes either an increase or decrease of *W′*, an increase if the anode is changed from the back to the palm and *vice versa*. This is apparently due to the secondary polarization differing in amount at the anode and kathode, and also differing in its effect on the skin of the palm and back of the hand. Veraguth states that the apparent resistance of the palm actually increases when current passes through it, while that of other parts of the skin (except the sole of the foot) decreases. If this is so, the effect of reversal on the hand resistance is explained if we assume that the secondary polarization change is more effective at the kathode than at the anode. Whatever may be its explanation, this phenomenon also points to the necessity for controlling the direction of the current. This necessity probably does not apply only to the hands, but in a lesser degree to all other parts of the skin.

(7) There remain a large number of factors influencing the size of *W′* which are not known and cannot therefore be controlled. Even after washing the skin with alcohol the apparent resistance of a subject varies from day to day with identical electrodes and under identical experimental conditions, and even from minute to minute in the course of a single experiment. There is no evidence that these differences are altogether indicative of general changes in the condition of the subject of any psychological interest. There is ample evidence that they are so in part, but they may also be in part due to differences in the skin condition of purely local significance. Particularly is this probable in the variations of *W′* from day to day. The usefulness in psychology of electrical measurements and particularly of measurements of *W′* would be much enhanced if we knew any method of entirely eliminating these variations.

VI. Preliminary problems.

The usefulness of the P.G.R. and the T-phenomenon as instruments of psychological measurement depends largely on the solution of some preliminary problems. We should like to be able to use the shape of the electrical response curve as an objective indication of the character of response of a subject to stimulus, so that unscientific descriptions in terms of popular emotional and conational terms (such as "he felt afraid" or "he decided to do so and so") may be replaced by objectively verifiable indications of the nature of the physiological pattern of response. We should like also to be able to give a quantitative measure of such responses.

It is clear that we need as full an understanding as possible of the physiological response before we can get very far in correlating forms of the curves of electrical response with different conational and emotional responses. Much work on the physiological side is being done, such as Densham and Wells's demonstration that changes in apparent resistance may follow from the constriction or extension of the skin with vasomotor changes (15). Yet it is difficult to believe that the physiological problems of the phenomenon are solved. The complex form of the T-phenomenon suggests the presence of different physiological components with different time relationships. If there are specific differences in the P.G.R. response to different situations, it seems too that these must be the result of differences in the action of different components in the physiological reaction. The most hopeful first step in the study of such specific differences would seem to be the separation of these different components.

Many investigators prefer to make a bold attack on this more complex problem without waiting for the success of any preliminary investigation. There is much to be said for this attitude. It must be remembered, however, that such a problem as that of specific differences in the forms of curves can only be attacked with the finest instruments. Curves for this purpose obtained with long period galvanometers or on unsatisfactory circuits are of no scientific value whatever.

The use of the electrical responses as indicators of the quantity of response also necessitates preliminary research into more simple problems. The simplest of all is a demonstration of the quantitative relationship between responses to controllable stimuli of different physical intensities. This is an important and very simple matter which has received surprisingly little attention. Then there is the question of whether

$\Delta W'/W'$ is a significant measure of the extent of the P.G.R. for different values of W'[1]. The settling of this question is absolutely essential to any comparison of sizes of P.G.R.'s except under the very unusual conditions that the P.G.R.'s compared have been measured at the same value of W'.

The problem of the physical nature of the P.G.R. phenomenon (whether a resistance or polarization change) has generally been regarded by psychological investigators as no concern of theirs. With some exceptions, the psychological investigator is concerned with problems in which this difference is unimportant. One of the exceptions is, as indicated earlier, that many of the factors in experimentation (such as the voltage and constancy of e) which would not need control if we were measuring a pure resistance change, need in fact careful control if we are dealing with a phenomenon of polarization. The other point at which the difference becomes important is the question of the significant measure of the P.G.R. The theoretically probable significant measure depends on the physical nature of the change. The physical nature of the change is, therefore, also one of the preliminary problems of the P.G.R. About the physical nature of the T-phenomenon there appears to be no dispute.

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[1] Mr R. B. Cattell has realized the importance of this problem and has published figures tending to show that $\Delta W'/W'$ is less for high values of W'(16). His circuit was not ideal for this purpose, and it is to be hoped that these figures will be confirmed on a circuit with adequate elimination of the T-phenomenon and with P.G.R.'s obtained from a physical stimulation of exactly controllable intensity.

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THE TRANSFER OF TRAINING[1].

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At present a somewhat paradoxical situation exists with regard to the problem of transfer. In the majority of experiments transfer has shown itself either not at all or only in a degree that may be described as statistically and theoretically significant but practically unimportant. Yet many psychologists are convinced of the practical importance of transfer. It would hardly be reasonable to accept without reserve an opinion not consistent with the bulk of the available experimental evidence, but the prevalence and strength of such a belief do seem to call for some sort of stocktaking. It may be asked whether experiment has clinchingly exhausted all the possibilities and whether the technique used has been completely satisfactory. The answer must, I think, be not only that these two requirements have not been met even in a limited practical sense, but also that some of the methods used have been such as to diminish the likelihood of finding any transfer at all.

Transfer investigation originated from the desire to test the doctrine of Formal or Faculty Discipline. A technique suitable for this purpose was developed and an experimental decision reached. After the rejection of the view that there are general mental powers which can be improved by practice in one field and which can carry over this improvement to other fields, there came attempts to re-state the belief in transfer in a form that implied no belief in Faculty Training. Experiments were set up to test these revised beliefs, but these experiments were framed on the general plan of the earlier work. This was unfortunate because the Faculty Training Technique is unsuitable for the solution of modern transfer problems. In what this unsuitability consists will appear presently.

If transfer takes place at all and if it does not take place through the medium of general mental powers, then it is natural to consider it as occurring between mental functions, that is, between such systems of habits, such products of learning as skills or abilities. (One speaks of the ability to typewrite or the skill of typewriting or of the mental function of typewriting, the different terms emphasizing different aspects of substantially the same entity.)

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[1] This paper was read before Section J at the South African Meeting of the British Association, July, 1929.

Each such ability is no doubt based upon innate factors; but as it expresses itself in actual behaviour and experience, it is essentially a learnt system, an acquired ability. Each such ability may further be thought of as consisting of a set of constituent abilities or functions, integrated and organized in a particular way. Thus arithmetical ability may be thought of as consisting of the ability to add, the ability to multiply, and the like; and these constituent abilities can conceivably be analysed into the ability to multiply 2 by 3, and so on. It is not of course implied that such constituent abilities are identical when functioning as constituents of different abilities, that is, in different settings. Part of the problem is to discover how far there can be identity in such cases. Nor is it implied that the analysis of an ability into its constituents can be satisfactorily carried out in any but an experimental way. The analysis of arithmetical ability sketched above is for illustrative purposes only and is not guaranteed. But pending the development of a method of analysis, it may be permitted to think of abilities in the way described in so far as the thinking leads to an experimental issue.

Abilities then are provisionally to be thought of as made up of constituent abilities, co-ordinated on a particular plan, without any implication that either the pattern of organization or the abilities organized are independent of one another. As constituents may be included such abilities as that of taking up an active problem-solving attitude, that of resisting certain distractions, that of working up to a certain level of achievement, and the like. Whether constituents differ in kind as well as in complexity and specificity, is not material here.

It is the common practice to designate abilities by the product produced, the function performed or the purpose served. As a name, as an indication of the object of thought, this is useful; as a description it is clearly inadequate. Thus one person's ability to typewrite and another person's ability to typewrite might be thought of as differing only in degree, not in kind. Both produce typescript. Yet one may use the touch method, the other the sight method; one may use both hands, the other only one. These are gross differences, but even in the absence of such obvious differences it cannot be argued that differences do not exist or that existing they are rare and unimportant. A better plan would be to describe abilities in terms of their constituents and of the mode of organization of the constituents within the ability. It would not be possible to do this very fully as yet. But to attempt to do it would at least save one from thinking of abilities identical in purpose or product as identical in all other respects.

The training of an ability may involve changes in the constituents—constituents being omitted or modified and new ones introduced; and it may involve changes in the organization of the constituents. Both kinds of changes, changes in constituents and changes in organization, are qualitative changes. They may be distinguished collectively from that quantitative change which results from training and which consists in a facilitation change, in a change in the working of the constituents. A facilitation change does not necessarily involve any qualitative change; in fact, however, training is apt to bring about qualitative as well as quantitative changes, and it should never be assumed without proof that the effect of training is merely a facilitation change. Adequately to describe the effect of training would be to describe qualitative as well as quantitative changes.

Usually the effect of training, being thought of mainly or solely as a facilitation change, is expressed in terms of functional efficiency and measured by change in the quantity of product per unit of time, by change in liability to error, and the like. Now such measures are reasonably adequate so far as facilitation changes are concerned. The effect of a qualitative change is, however, far less likely to be accurately measured in this way. The change in efficiency may be potential rather than actual. A change from the sight to the touch method of typewriting may at first result in a diminution of efficiency and any potential superiority in respect of a higher limit of attainable efficiency may not manifest itself until much more training has been given. Generally it may be said that a qualitative change in an ability is less likely to be immediately and adequately reflected as a change in efficiency than is a facilitation change. Time is needed for the potential change in efficiency to actualize itself.

It is now possible to re-state the problem of transfer. Transfer of training refers to those changes (if any) in the facilitation and in the constituents and organization of an ability which result, not from direct training of that ability but from training some other ability. In other words, it refers to those qualitative and quantitative changes in one ability which result from qualitative and quantitative changes in another ability. The problem of the transfer of training concerns itself with the nature, magnitude, extent and direction of such changes and with the conditions which determine them.

Adequately to investigate such a problem or set of problems it is clearly necessary to choose very carefully the abilities between which transfer is to be expected; for it is certain that transfer does not occur in appreciable amounts between any two abilities, and as transfer does seem to occur in certain cases, it would seem advisable to work outwards from the investigation of these cases rather than to cast one's net at random. Further, the abilities chosen ought to be described in regard to their qualitative as well as to their quantitative state. The training of the transferring ability should be very strictly regulated and observed, and its qualitative and quantitative development described. Finally long and closely regulated and observed training tests should be used for the purpose of detecting transfer effects, both for the control and the training groups. And search should be made for qualitative as well as for quantitative differences between the two groups.

An experimental technique satisfying these general requirements certainly does not exist at present and will take long to develop. In the meantime these requirements should be borne in mind when transfer experiments are being planned. Yet how often are the abilities chosen almost at random and defined merely in terms of product or function or purpose! How often is the training essentially unregulated and unobserved, and its effects measured only by short immediate efficiency tests! Such considerations are negligible when it is a question of the doctrine of Formal Discipline. To neglect them in transfer experiments is to imperil the whole experiment. For if one takes a group of individuals and limits them in regard to little more than kind of product and amount of time spent in learning to produce that product, it is probable that at the end of the training the group will be far from homogeneous in respect of the ability trained, whatever may have been the case at the outset. They will produce the same kind of product but they will do it in different ways, in other words there will be qualitative differences between abilities nominally only quantitatively different. Without close observation of the training and minute description in terms of constituents and pattern of organization, the nature of these differences will be unknown. Further, if brief immediate efficiency tests, which ignore qualitative changes, are the sole indicators of transfer effects, the possibility of any transfer at all being found is significantly reduced.

It has already been said that the problem of transfer concerns itself with the changes in constituents, organization and facilitation that occur in one ability as the result of changes in another ability in these respects. It will perhaps be helpful if a brief survey of the field of investigation is made from this standpoint. This survey, necessarily brief, deliberately concerns itself with those cases where transfer has actually been found or where it may reasonably be expected. No attempt is made to discuss the practical importance of transfer.

First may be considered the case of facilitation changes in Ability A due to facilitation changes in Ability B. This coincides roughly in scope with the Theory of Transfer by Common Elements in its positive form. In its most general positive form that theory assumes that if there are constituents common to two abilities, and that if one of the two abilities is modified in regard to its state of facilitation by practice, then the other ability will be modified in virtue of the common constituents[1]. It seems probable however that this statement is not unrestrictedly true, that at least the magnitude and time of incidence of the transfer will vary with certain conditions. If for instance the increase of facilitation in Ability A refers rather to the working together of the constituents than to any improvement of the individual constituents, if the facilitation is one of the integration of the constituents, then unless not the simpler constituents but at least one organized set of simpler constituents is common to the two abilities, there seems no reason to expect transfer to occur.

Generally the problem is one of facilitating by practice one ability and ascertaining under what conditions the facilitation transfers to another ability containing some of the same constituents or at least some that are overtly the same. Many kinds of cases are possible. The constituent abilities can be trained separately before combination in one inclusive ability. Or the reverse case can be taken—the inclusive ability formed first and then the constituents taken alone. Or the case of constituents included in two different abilities. In all these cases it may be questioned how far the amount of transfer is likely to be independent of the stage of learning reached and the strength of the integration of the constituents in the abilities concerned.

The problems involved are perhaps less obscure where the one ability is in process of formation and incorporates constituents of the other more or less trained ability. It seems probable that in such cases considerable re-adjustment will be required—the transferred constituents have to be integrated into their new setting, organized into a new pattern and with new associations, the process being accompanied by more or less interference from the old setting. It seems likely that time will be needed for any potential transfer to be actualized, for integration and re-organization to reach a level at which efficiency rises above zero or above the initial score, and for interference to abate. Both time and practice are necessary for the manifestation of the transfer. One illustration of this may be seen in Pyle's card-sorting experiments. His Group B, after 15 days' practice with one arrangement of the boxes (Scheme 1), was asked to sort the cards with a different arrangement (Scheme 2). On day 1 (Scheme 1) their average time per sorting was 750 seconds, on day 1 (Scheme 2) it was 325 seconds; and day 15 (Scheme 1) was equalled by day 4 (Scheme 2) with 150 seconds per sorting[1]. Thus the full amount of transfer did not appear till the fourth day, and although considerable transfer did appear on the first day, it must be remembered that the initial score for Scheme 2 is the average score for the whole of the first day's work. Had the score for the first sorting been given instead, it is probable that the amount of immediate transfer to be recorded would have been appreciably diminished.

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[1] I do not here raise the question of the extent to which one can safely argue from the overt similarity of constituents to adequate functional identity of the underlying psychophysical processes. But clearly unless sufficient identity can be assumed, i.e. identity in so far as transfer is concerned, the basis of the theory disappears. The assertion that common elements exist, or at least that there is community of function adequate for transfer purposes, is fundamental. But it seems to me that this assumption is better tested by the sort of investigation indicated above than in any other way. From one point of view investigation in this field is an enquiry into the existence and extent of the underlying identity of overtly similar constituents.

Another illustration is given by one of Book's subjects (Subject X). This subject, after 85 hours' practice at typewriting by the sight method, had reached a speed of 1600 strokes per minute from an initial speed of 300 strokes per minute. After a five months' interval the subject restarted typewriting, this time by the touch method. The initial speed was less than 200 strokes a minute and a speed of 1600 strokes a minute was not reached till after 60 hours' practice. In this case it seems likely that no positive transfer would have shown itself in an immediate brief test. As it was, a saving of about 25 hours appeared[2].

Cases capable of similar interpretation are not uncommon. The ability with which we respond to some more or less novel situation is frequently a modification of some already formed ability or contains constituents practised in other settings. In such cases more or less re-adjustment is necessary, more or less interference is operative, and the full amount of the transfer only appears when the re-adjustment is complete and the interference has abated. In this way can be explained the very rapid early improvement found in some cases of learning—for instance, Thorndike's finding that as a result of 7 hours' practice the ordinary college graduate will diminish the time required to do mental multiplications to two-fifths of the initial time[1]. Thorndike, after reviewing a number of cases of the learning of abilities under experimental conditions, draws attention to the frequency with which a very rapid rate of improvement occurs[2].

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[1] W. H. Pyle, "Transfer and interference in card-distributing," *J. of Educ. Psychol.* 1919, x, 107–110. The figures given are read off to the nearest 25 from Pyle's Fig. 1. Pyle's experiments were performed for a purpose somewhat different from that of the text and illustrate rather than establish the interpretation given here. The same remark applies to the other examples given in this paper.

[2] The facts are taken from E. L. Thorndike's *Educational Psychology*, II, 138–139, Figs. 58 and 61. That the drop to 200 strokes a minute at the beginning of the touch typewriting was not due to forgetting, is, I think, clear from the facts about X's forgetting given on p. 311, Table 31, as well as from other considerations.

How much of the lag of the transfer is due to interference, and how much to re-adjustment apart from interference, is not clear at present, and for most purposes it is convenient to lump them together without any attempt at separation. But certain general considerations appear. It would seem that interference is maximal where the transferring and recipient abilities, and the situations to which they are adjustments, differ very little. When situation, pattern, and all constituents save one are identical, they will all condition a strong facilitatory tendency towards the constituent with which they have been previously integrated and a strong inhibition against the new constituent with which they are now to be integrated. The learner combats this by giving the new constituent a mental prominence which, while necessary at the outset in order to overcome the interference, is only temporary, and has to be modified later on if the most effective ability is to be developed. The more the abilities differ in situation, pattern and constituents, the less strong the interfering tendency and the greater the necessity for re-adjustment. The transferred constituents have to be integrated together with new constituents into a new pattern adapted to a modified situation.

Newkirk and Gundlach[3] have recently carried out an experiment which bears on this question. They have shown that training in the cancellation of 6's produces a significant positive transfer to the cancellation of *d*'s but not to the cancellation of 2's. A short test was used as the indicator, and so it is not possible to say decisively whether or not there

[1] Cf. E. L. Thorndike, *op. cit.* p. 148. I refer here to cases where the initial improvement is uncommonly rapid. How far the negative acceleration general in learning curves is due to the factors of transfer and interference and re-adjustment is not dealt with here. It might be worth investigation.

[2] E. L. Thorndike, *op. cit.* pp. 150 and 157.

[3] "The transfer of training in cancellation experiments," *J. of Educ. Psychol.* 1929, xx, 291 ff.

was potential transfer in the case of the 2's. As, however, in spite of the interference that existed, the training group did no worse than the control group, it seems probable that a long test would have demonstrated transfer in the case of the 2's. The amount of interference in the case of the *d*'s must have been much less, for the figure blank contained only figures and the letter blank only letters. Had the blanks contained both figures and letters, there would have been interference between the tendencies to strike out a *d* and to ignore it and between the tendencies to strike out a 6 and to ignore it, and this interference would have been all the greater owing to the similarity of the situation, pattern and other constituents. Just as there was in the case of the 6's and 2's.

So far in this paper the discussion has been confined to the set of problems relating to the extent to which, and the conditions under which, facilitation changes in one ability can produce facilitation changes in another ability, given common constituents. Another and not less important set of problems arises in regard to the question, How far and under what conditions can qualitative changes in the one ability produce qualitative changes in another ability? How far does the nature of the constituents and of their organization in the one ability determine the nature of the constituents and of their organization in another ability[1]? If such determination occurs, it should ultimately if not immediately express itself in an efficiency change. But as efficiency changes of themselves give no indication of their causation, and as the determination will manifest itself immediately as well as ultimately in qualitative changes, attention should be primarily directed to the qualitative changes in both the transferring and the recipient abilities.

In adjusting to a situation that is significantly novel to a greater or less extent, it is frequently assumed that the learner tends to apply abilities already acquired and in some sort of relation to the similarity between the new situation and old situations to which adjustments have already been made, to the degree of familiarity of the old situation and to the degree of facilitation of the ability or response adjusted to that situation. How far the ability in process of formation is permanently modified by such tentative adjustments depends upon many conditions, principally upon whether or not the old ability can be made to serve the new purpose sufficiently well.

But initially everything depends upon whether or not the new situation evokes the old ability. There are many cases where the resemblance between the new situation and the old is obvious, or, to put the matter another way, where the probability is great that the new situation will evoke the same response as the old or at least a response containing some of its constituents. There are also cases where the probability is less great, where a smaller proportion of learners would make the connection. What are the conditions which determine the probability of making the connection?

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[1] I shall not discuss save incidentally either the production of qualitative changes in one ability by facilitation changes in another ability or the production of facilitation changes in one ability by qualitative changes in another ability.

It seems likely that there will be a positive correlation between amount of intelligence and the extent to which such connections are made[1]. But it also appears likely that the probability of making such connections can be increased by modifying the transferring ability. This modification will be in the direction of generalizing the ability, that is, of narrowing down the situation to which the response is made, to the more essential specific features so that a greater number of situations can evoke the response, and possibly of narrowing down the response to the more essential constituents so that it can be used with new constituents at a smaller price in re-adjustment and interference. Greater range is secured by making the situation and response less precisely adapted in detail to any one concrete situation.

This end is achieved by pointing out cases where an ability in process of formation can be applied, by teaching general rules of application, by emphasizing the essential aspects of the situation and the response, and generally by making the methodological aspect as explicit as possible. But to do this is to produce an ability qualitatively different from one produced by a less generalized training. The pattern of organization will be different and there will be differences in constituents. But, if this is the case, efforts should be made to describe these differences directly. That they will ultimately express themselves in efficiency changes is insufficient warrant for observing only efficiency changes, particularly if short, immediate tests are used. To do this is to jeopardize one's chance of getting any efficiency changes at all and to forfeit one's chance of explaining the absence or presence of efficiency changes.

It is in the direction of transfer by generalization that the believers in transfer have looked for the most important results. And the few experiments which have been carried out in this field seem to support their expectations. The experimental evidence would be much stronger if the precautions and principles advocated in this paper had been at all generally observed. For to demonstrate transfer, when the dice are loaded against success, is of good augury for the future.

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[1] The relation between transfer and intelligence still awaits adequate investigation.

To illustrate: Ruediger inculcated the practice of neatness in connection with a certain school subject and in addition generalized the training by teaching the ideal of neatness. He obtained indications of transfer, whilst Squire, who refrained from any such generalization or explicitness, obtained none[1].

Woodrow trained two groups in the memorization of poetry and nonsense syllables. To the one group was given instruction in the best methods of memorization, practice in applying such methods to the memorization of poetry and nonsense syllables, and emphasis upon the desirability of applying such methods to other kinds of material. The group so instructed showed a clear gain over the control group in short immediate tests, whilst the group which had been left to its own devices during the period of training showed no such clear advantage[2].

In Judd's well-known experiment two groups, one of which had received instruction in the principles of the refraction of light, were trained in throwing darts at a target below water. No difference was observed between the two groups in acquiring this ability, but when the depth of the water was altered the instructed group quickly outstripped the other[3].

These three cases (taking Ruediger's and Squire's investigations as complementary) enforce the necessity for qualitative observation. The abilities given the generalized training were in all probability qualitatively different from the abilities not given the generalized training. Certainly they reacted differently to transfer opportunities. It is reasonable to suppose that close observation directed to discovering qualitative differences between the groups would not have been completely fruitless. Qualitative as well as quantitative differences should be looked for between the groups receiving different kinds of training during as well as at the end of their training, and between the training and control groups during as well as at the end of the training of the test abilities.

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[1] E. L. Thorndike, *Educational Psychology*, II, 411–412.

[2] "The effect of the type of training upon transference." *J. of Educ. Psychol.* 1927, XVIII, 159–172.

[3] E. L. Thorndike, *op. cit.* pp. 400–401.

SUMMARY.

1. The problem of the transfer of training should be regarded as that of investigating the conditions under which qualitative and quantitative changes in one ability due to training determine qualitative and quantitative changes in other abilities.

By qualitative changes are meant changes in the nature of the constituents and of the organization of an ability. By quantitative changes are meant facilitatory changes in the ability. Both kinds of changes may manifest themselves as efficiency changes.

2. The abilities investigated should be carefully chosen, on the ground that transfer does not occur anywhere and anyhow but only when certain not yet fully known conditions are satisfied.

3. Transfer should be sought for in long training tests, not in short immediate tests. This is necessary in so far as efficiency is concerned, because some of the transfer may not immediately manifest itself in efficiency changes and because efficiency gains are liable to be masked at first by temporary efficiency losses due to re-adjustment and to interference. But it is also necessary in order to ascertain the qualitative transfer changes and to determine the causes of the efficiency changes.

4. The training of the transferring and recipient abilities should be closely observed and regulated, and attempts should be made to determine qualitative as well as quantitative ability differences between groups given different kinds of training as well as between training and control groups.

(*Manuscript received* 18 *October* 1929.)

A NOTE ON OVER-TRAINING.

By EDNA M. HENSHAW and PORTIA G. HOLMAN.

(From the Department of Psychology, University of Manchester.)

The following results on the effect of length of practice period arose incidentally from experimental work on the acquisition of manual dexterity. Three groups of 30 subjects were employed on chain assembling[1] for a training period of 80 minutes each morning. In the afternoon Group I had a further 80 minutes' chain assembling. Group II had 80 minutes' cartridge filling[2] and Group III was not employed.

Thus the morning occupations of the groups were identical, and the afternoon employment is the one variable.

The three groups were employed for a fortnight. The comparison between their practice curves in the morning chain assembling is shown in Fig. 1.

It will be seen that their performances were almost identical in spite of the fact that Group I had actually twice as much training as Groups II and III.

After a period of some months, five subjects from Groups II and III were re-employed for a fortnight, in which they continued training in chain assembling for 80 minutes each morning[3]. After a drop due to the interruption of practice, this group continued to improve at the same rate. This shows that maximum improvement had not been reached earlier, and that the failure of the concentrated practice of Group I to effect improvement was not due to any flattening of the practice curves. When these five subjects had had the same total amount of training as Group I, their output was very much higher.

These results show that increase of the daily training period beyond a certain length has no apparent effect. The extra 80 minutes' training in the afternoon resulted in no additional improvement whatever.

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[1] For a description of the actual operation of chain assembling see Langdon and Yates, this *Journal*, April, 1928, XVIII, Part 4.

[2] Cartridge filling was specially designed to involve movements as like those of chain assembling as possible though using different material.

[3] This interruption was due to an unavoidable necessity for the main purposes of the experiment from which the results here reported were obtained.

It is worth noting that each of the 80-minute training periods was divided up into eight 10-minute periods separated by 10-minute rest pauses. Moreover, the two 80-minute training periods of Group I were separated by a luncheon interval of an hour and a half. No appearance of fatigue was shown either in the mornings or the afternoons. The fact that Group I maintained as high an output in the afternoons as in the mornings and that Group II improved steadily in cartridge filling shows clearly that fatigue was not operative.

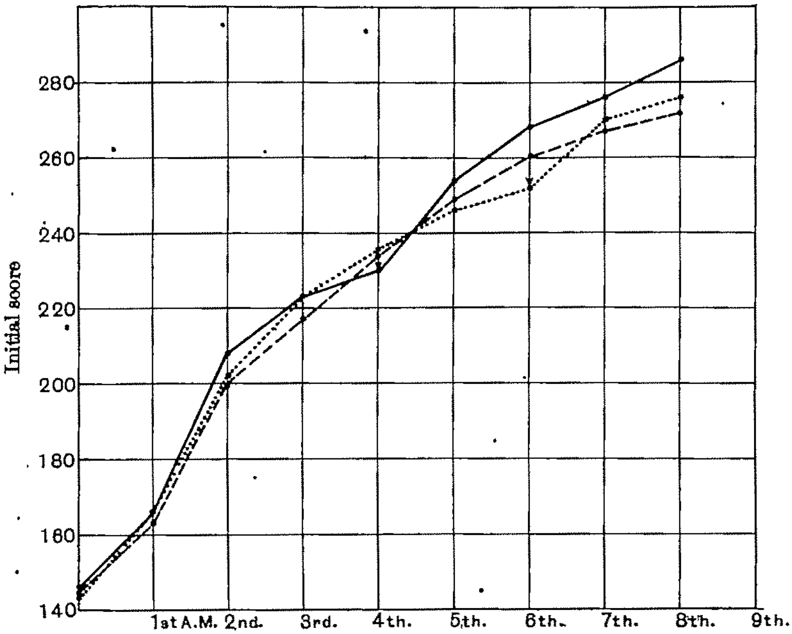

Fig. 1. Practice curves of chain assembly groups.

——— = Group I. Chain assembling A.M. and P.M. Sixteen 10-minute periods per day chain assembling.

•••••• = Group II. Chain assembling A.M. Cartridge filling P.M. Eight 10-minute periods per day chain assembling; eight 10-minute periods per day cartridge filling.

------ = Group III. Chain assembling A.M. Rest P.M. Eight 10-minute periods per day chain assembling.

These results further emphasize the importance of determining the optimum arrangement of training for any particular operation. It is evident that 160 minutes' training per day is well above the optimum for chain assembling, and probably for other similar operations. In fact, the 80 minutes' morning training is probably excessive.

In this comparison, *length* of daily training period has been the variable under consideration. How far the relative rate of improvement is affected by *distribution* of training is not shown in these results, but the relation between length and distribution of training period is a problem with important practical consequences, and one which could be solved by further experimentation along the same lines.

(*Manuscript received* 20 *December*, 1929.)

AN EXPERIMENTAL INVESTIGATION INTO THE SIMULTANEOUS CONSTITUENTS IN AN ACT OF SKILL.

BY C. E. BEEBY.

(*From the Manchester University and University College, London*[1] *Psychological Laboratories.*)

I. *Statement of problem* (pp. 336–338).
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III. *Consciousness and the acquirement of skill* (pp. 350–352).
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IV. *Summary and conclusions* (pp. 352–353).

I. STATEMENT OF PROBLEM.

THE relation of 'parts' and 'wholes' first gained psychological, as distinct from philosophical, significance, in the work of Lottie Steffens on the economy of verbal learning[2]. The problem was shifted from the verbal to the muscular sphere by the researches of Pechstein[3] and Gopalaswami[4] upon part and whole methods in maze-running and mirror-drawing respectively.

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[1] My thanks are due to Professors Pear and Spearman for help and guidance in the planning of these experiments. This paper, moreover, was written in New Zealand, and its passage into print would have been impossible but for the further kindly assistance of Prof. Pear.

[2] *Zeitschrift für Psychologie der Sinnesorgane*, 1900, p. 321.

[3] *Psychological Monographs*, 1917, XXIII, No. 2.

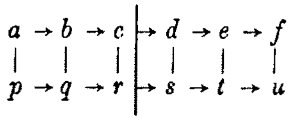
[4] This *Journal*, XV.

But there is one great difference between the 'part-whole' problem of verbal learning and that of muscular skill, which has never been touched upon: the former is one-dimensional, the latter two-dimensional. A mental process is a unitary stream which can be divided only into *successive* constituents. A muscular process, on the other hand, can be split into simultaneous as well as successive constituents.

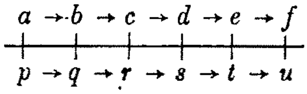
Suppose, for example, that the series of muscle, joint and tendon changes which result when I raise both hands from this page is:

$$\begin{tikzcd}
a \arrow[r] \arrow[d, dash] & b \arrow[r] \arrow[d, dash] & c \arrow[r] \arrow[d, dash] & d \arrow[r] \arrow[d, dash] & e \arrow[r] \arrow[d, dash] & f \arrow[d, dash] \\
p \arrow[r] & q \arrow[r] & r \arrow[r] & s \arrow[r] & t \arrow[r] & u
\end{tikzcd}$$

where the top line represents the movements of the left hand, and the bottom line those of the right. A division of this movement-whole may take either this form:



or this:



The former we have termed 'Successive Division,' and the latter 'Simultaneous Division.' The corresponding reverse processes of building up more complex movement-wholes from simpler constituents are 'Successive Combination,' and 'Simultaneous Combination.'

Successive and simultaneous constituents are, of course, by no means independent of one another. In all activities where more than one muscle is contracting at a time, simultaneous constituents are necessarily present: whenever an action is spread out over a period of time, Successive Division is possible.

This paper confines itself to the relation between simultaneous constituents. Its problem is twofold:

(*a*) What happens in Simultaneous Combination? Does any movement, when combined with one or more other movements, lose, to some extent, its own character, and take on another character due to the combination? If so, what are the changes that take place?

(*b*) What happens in Simultaneous Division? Does a movement, when divided, resolve itself into a number of simpler movements, which bear exactly the same relation to one another in isolation that they did in combination? If not, what differences may we expect?

These questions are not open to any direct form of test, since no apparatus is available for measuring exactly the position and tension changes of all the muscles, joints and tendons in a complete and living member. Our only appeal, therefore, is to the relative efficiency of the movement before and after division or combination. An action is practised until a known degree of skill has been attained: it is then subjected to division or combination, and the efficiency of the agent again tested. If all other changes can be excluded, any loss of skill indicates a change in the structure of the original movement.

II. Experimental.

(*a*) *Apparatus.*

Fig. 1 is a diagrammatic sketch of the apparatus used. It consists of a flat wooden base (*A*), 55 × 27 cm., mounted on short rests. Pivoted on this board, in a horizontal position, are two strong wooden discs (*B*), so attached that they can be revolved about their centres, and locked in any position. These discs are pierced by holes which allow of the affixing of a variety of metal shapes. These consist of a number of geometrical figures, of differing shapes and sizes, cut in outline from $\frac{1}{16}$-inch brass. The strips forming the outline of the figures are uniformly 1 cm. in width. They are attached to the discs by means of screws sweated on their under sides.

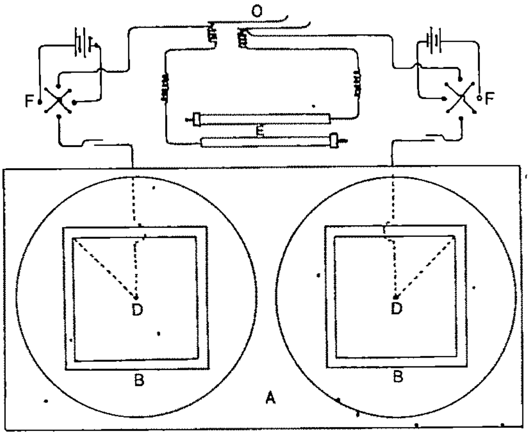


Fig. 1. Diagrammatic sketch of Figure-Tracing Apparatus.

A = Wooden Base. *B* = Wooden Discs. *C* = Time Markers.
D = Centre Bolts. *E* = Stiluses. *F* = Commutators.

Squares only were used in the experiments referred to in this paper. (Further experimentation involved various combinations of figures.)

The task is to follow around the brass squares with metal stiluses (*E*), in such a way that the contact between stilus-tip and figure is broken as seldom as possible. Both hands move in an anti-clockwise direction. Each break constitutes an error, and is registered by time-markers on a paper-tape kymograph. There is a separate circuit for each hand. The sides of the squares are parallel with the edges of the baseboard. The subjects are blindfolded.

Simultaneous Combination occurs when the right and left hands, having been separately trained in the tracing of the squares on the right and left sides of the apparatus respectively, are forced to perform these movements together. Division is represented by the reverse of this process.

(*b*) *The groups.*

In order to answer the above questions at least six groups of subjects were necessary. With one or two exceptions they were all advanced students in psychology. The subjects were divided into two main groups. Group A commenced with the single-handed process (Simultaneous Combination), and Group B with the double-handed (Simultaneous Division). Group A returned to the single-handed after undergoing double-handed practice, and Group B returned to double-handed after the intervening period of practice on the single-handed process. A measure was thus gained of the amount of positive or negative assistance given by one type of practice to the other.

Groups A and B were further subdivided as shown in Table I. Each group was composed of four subjects. The distinction between Groups A-a and A-b, and between B-x and B-y was necessary in order to show to what extent there is a transfer of skill from practised right hand to unpractised left, and *vice versa*. Without a knowledge of this factor we cannot compare the performances of the A groups before and after double-handed practice.

The Control Groups, A-c and B-z, were rendered necessary by the fact that each subject's performance took from two to three hours. To avoid fatigue, the test was given in two sittings. Seven days elapsed between the sittings. The division, of course, was never made at any of the crucial points of change from Double to Single, and *vice versa*. But the mere lapse of time, however short, might be sufficient to invalidate our comparisons between the original task and the return to it in the third part of the experiment. Hence the necessity for the Control Groups, where the lapse of time was the only factor intervening between the first and the third parts.

(c) *Analysis of results.*

Figs. 2 to 7 give the results for the above-mentioned groups. The trials were grouped in sets of ten, so that each reading represents the total of ten trials. The total time taken (in seconds), and the total number of errors made by each individual in doing each set were found; and the average of these readings for the four subjects comprised in each group gave the figures on which the graphs are based. The Time Error (T.E.) score is merely a composite of the scores for time and errors.

There are four main problems involved in the study of our graphs:

(*A*) The behaviour of the curves at the first change, *i.e.* from Single to Double in the A Groups, and from Double to Single in the B Groups. This crucial point is marked by a vertical broken line in the graphs.

(*B*) The behaviour of the curves at the second critical point, where the subjects return to their original tasks. This is denoted in the graphs by the vertical dash-and-circle line.

(*C*) The amount of transfer of skill from the Single to the Double operation, and *vice versa*. This can be found only by a comparison of the levels attained by the A Groups and the B Groups in the various processes.

(*D*) The amount of transfer from one hand working singly to the other, also working singly.

(*A*) *First change.*

Fig. 2, which represents the process of combination, shows a considerable rise in both time and error curves at the point of transition from the single to the double action. The same phenomenon is found, to an even more marked degree, in Fig. 3, which is the same process commenced with the left hand. There is nothing in these graphs, however, to indicate that any of this loss of skill is due to the breaking down of two movement-wholes and the necessity for the construction of a third distinct motor-whole. It might quite possibly all result from the inherently greater difficulty of the double-handed performance.

This is proved not to be the case by the records of the corresponding B Groups (Figs. 5 and 6). For here, although the change is from the inherently more difficult to the inherently less difficult action, there is still a sharp rise in the curve of errors at the point of transition.

The improvement in the time curves is of no significance. It is practically meaningless to compare the *speed* of the single-handed movement with that of the double-handed, since, in the latter, there are considerable periods during which one hand remains stationary, whilst the other fumbles blindly in an attempt to find its bearings. There are two factors affecting the time curve in passing from Single to Double, or *vice versa*: (*a*) the loss of time caused by one hand having to wait for the other in the double-handed work, (*b*) the loss of efficiency which we anticipated would result from combination or division. Both factors work in the same direction in the A Groups (Single to Double), and in opposite directions in the B Groups (Double to Single). In the A Groups the average increase in time at the first change is 148 sec. (L.H.), and 146·5 sec. (R.H.). In the B Groups there is a decrease in time of 23·1 sec. (L.H.), and 27·4 sec. (R.H.). That is, factors (*a*) and (*b*) almost neutralize one another in the B Groups, proving that nearly half the decrease in speed at the change from Single to Double in the A Group is due to loss of efficiency resulting directly from the act of combination itself.

A comparison of the curves for the A and the B Groups, then, leaves no room for doubt that a transition from practised Single to unpractised Double, or from practised Double to unpractised Single always results in a great loss of skill. Since this loss occurs in both directions, there is no possibility of its being caused by the inherently greater difficulty of either action.

(*B*) *Second change.*

The results here are even more striking. We are dealing now with the subjects' return to the process with which they originally started. The points we are comparing are the ones immediately preceding the vertical broken line, and the ones immediately following the vertical dash-and-circle line.

It might well be expected that a subject already trained in the single-handed process would gain in single-handed skill from a lengthy period of practice with the two hands working together. Even more confidently would one predict that single-handed practice could do nothing but add to the skill already attained in the double-handed performance; for we approach most of our complex activities by preliminary practice on their analysed-out constituents.

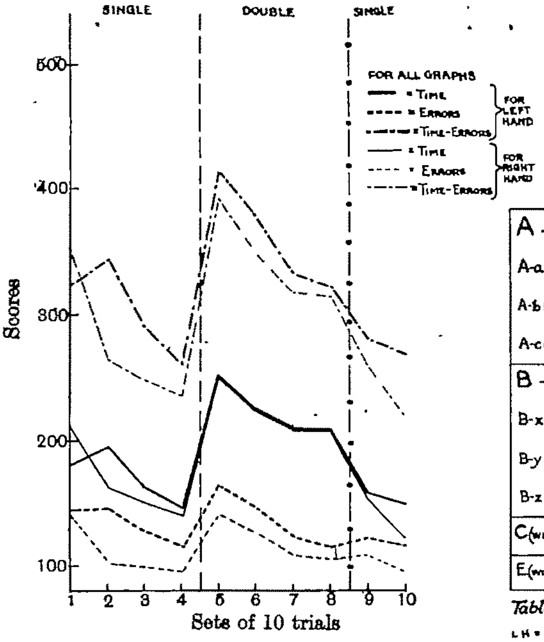


Fig. 2. Group A-a.
Single (R.–L.)—Double—Single (R.–L.).

A →	SINGLE followed by	DOUBLE followed by	SINGLE
A-a (Fig 2)	RH (40) LH (40) —	(40) —	RH (20) LH (20)
A-b (Fig 3)	LH (40) RH (40) —	(40) —	LH (20) RH (20)
A-c (Fig 4)	RH (40) LH (40) —	NIL —	RH (20) LH (20)
B →	DOUBLE followed by	SINGLE followed by	DOUBLE
B-x (Fig 5)	(40) —	RH (40) LH (40) —	(20)
B-y (Fig 6)	(40) —	LH (40) RH (40) —	(20)
B-z (Fig 7)	(40) —	NIL —	(20)
C (with vision) as for A-b D (with vision) as for B-y			
E (with altered mental attitude) as for B-y			

Table 1 — Showing Grouping (R H = Right Hand L H = Left Hand e g RH(40) = 40 trials with right hand)

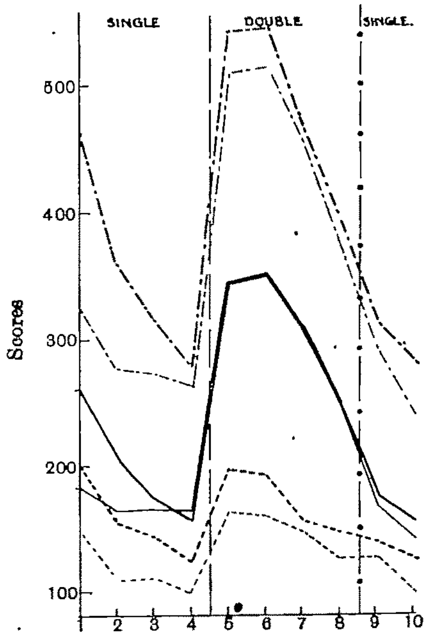


Fig. 3. Group A-b.
Single (L.–R.)—Double—Single (L.–R.).

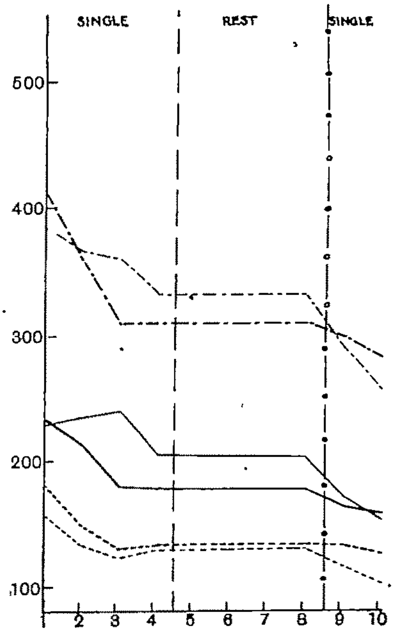


Fig. 4. Group A-c.
Single (R.–L.)—Rest—Single (R.–L.).

Exactly the reverse appears to be the case in the results we have obtained. There is not a single case in all the averages for Groups A-a, A-b, B-x and B-y in which the return to the original process, be it Single or Double, is not accompanied by a marked rise in both time and error curves. In the case of the return to the single-handed process (A Groups), the average T.E. of the left hand rose from 268·5 to 294·3, and of the right from 247·2 to 274·8. The corresponding figures for the double-handed process (B Groups) were: L.H. 302·6 to 344·2; R.H. 291·1 to 319·7.

We cannot conclude, however, on the basis of these results alone, that there is any interference between the performance of the single and the double action. The loss of skill in the third part of the experiment may be due simply to the time that has elapsed between the first and the third parts. Hence the need for the Control Groups, A-c and B-z, in which the second part of the experiment was omitted altogether. Any loss of skill in these groups must be due to fading alone.

But the results show no loss: there is a decided gain in the case of both the single and the double processes (Figs. 4 and 7). In the right hand of Group A-c the T.E. curve shows an average improvement of as much as 44 points. This does not necessarily mean that there has been any 'consolidation' during the seven days' rest period, but only that the improvement during the first set of ten trials after the rest more than equals any loss of skill due to fading.

The conditions were the same for the Test Groups as for the Control Groups. The only factor present in the former and absent from the latter was the interpolation of a different activity between the first and the third parts of the experiment, so that all the difference between the fall in efficiency in the Test Groups and the rise in efficiency in the Control Groups must be due to the 'interference' of the double process in Groups A-a and A-b, and of the single process in Groups B-x and B-y.

(*C*) *Transfer of skill between single and double operations.*

In spite of this, the A Groups, who came to double-handed work after practising single-handed, began the double-handed task at a higher level of performance than did the B Groups, who had had no preliminary single-handed practice. The B Groups, on the other hand, had the advantage in the single-handed task, which, with them, followed a period of double-handed practice.

Table II shows this clearly. The figures for 'Single Left' were got by subtracting the T.E. scores for Group B-y from those of Group A-b. 'Single Right' was got by subtracting the T.E. scores for Group B-x

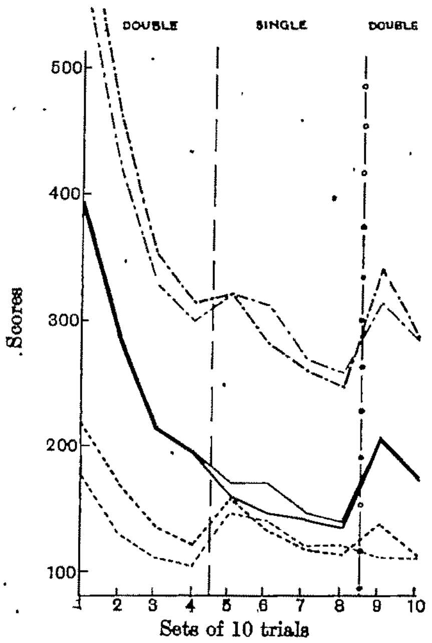


Fig. 5. Group B-x.
Double—Single (R.–L.)—Double.

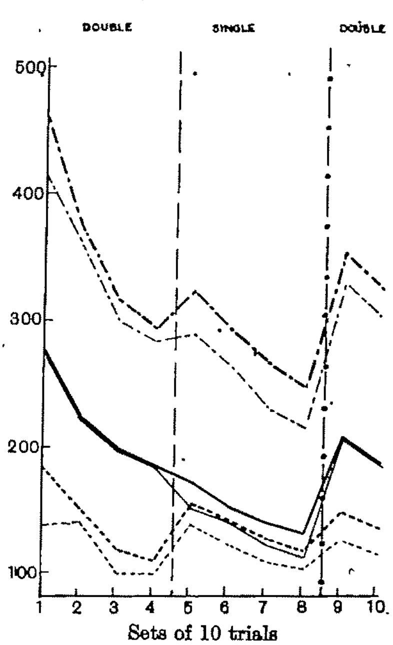


Fig. 6. Group B-y.
Double—Single (L.–R.)—Double.

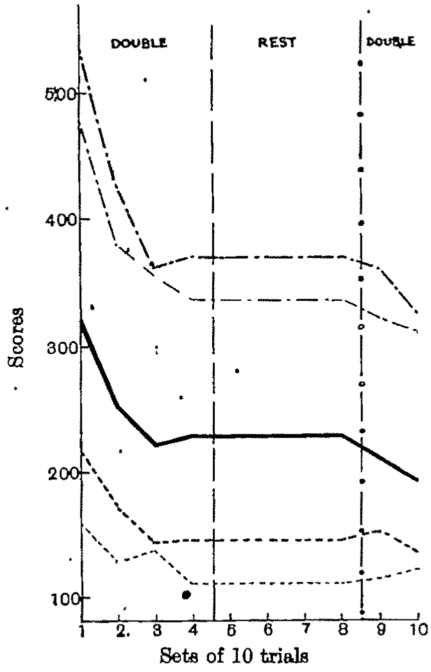


Fig. 7. Group B-z. Double—Rest—Double.

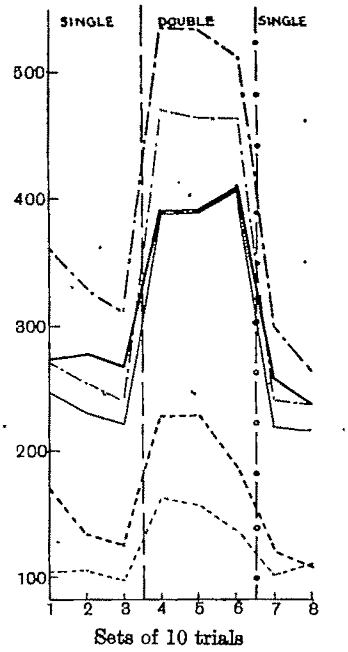


Fig. 8. Group C. With vision.

from those for Group A-a. Transfer from Single to Double presents a more difficult problem. The double action in the A Groups is never begun until both right and left have practised singly; and it is then impossible to say how much each has contributed to the final amount transferred. The nearest we have been able to do, in computing the 'Double' column is to subtract the average Double T.E. of the A Groups from the T.E. of the B Groups.

Table II. *Showing amount of transfer of skill between single and double operations.*

	Single		Double	
	Left	Right	Left	Right
Initial advantage of practised group	139·0	36·5	57·8	41·1
Final advantage of practised group	30·3	−24·9	−55·7	−55·2

A 'practised group' is one which comes to the single performance after practising the double, or to the double after practising the single.

An inspection of the T.E. marks for the first set of practices of the A Groups (Single to Double) and the fifth set of the B Groups (Double to Single) shows that those who came to the single performance after practice on the double had a marked initial advantage over those who started immediately with the single. The average gains are: L.H. 139 points; R.H. 36·5 points.

When we compare the fourth practice period of the A Groups, however, with the eight of the B's, we find that further practice has reduced this advantage of the 'Doublers' to: L.H. 30·3 points; R.H. − 24·0 points.

There is every indication that, if practice had been pushed a little farther in both groups, this advantage might have finally disappeared altogether in the left hand as it has in the right, even to the point of being transformed into a disadvantage.

However this may be, our results are sufficiently definite to lead us to suppose that the initial effect of previous double-handed practice is to give considerable help in the single-handed process, but that this advantage gradually diminishes as the single practice proceeds, until, possibly, it vanishes entirely.

This phenomenon is shown in a much more striking fashion in the 'Double' column. The A Groups started their double-handed performance with an advantage over the B Groups of 57·8 points of T.E. in the L.H., and 41·1 points in the R.H. After four sets of double-handed practice, the position is reversed: the B Groups now have the advantage, 55·7 points in the L.H., and 52·5 in the R.H. This means that the long period of single-handed practice has only served, finally, to hinder the acquisition of skill in the double-handed performance. Important practical conclusions would follow from such a principle.

(*D*) *Transfer of skill as between single hands.*

The question of the amount of transfer of skill from one hand to the other is of interest to us here only as it serves to throw light on the nature of the transfer between the single- and the double-handed operations. Table III shows, in parallel columns, the amounts of transfer from Double to Single, and from one hand to the other.

Table III. *Showing amount of transfer of skill between single and double operations, as compared with transfer from hand to hand.*

	Transfer from Double to Single		Transfer from hand to hand	
		T.E.		T.E.
Initial	Double to Left	139·0	Right to Left	146·8
	Double to Right	36·5	Left to Right	29·5
	Total: Double to Single	175·5	Total: hand to hand	176·3
Final	Double to Left	30·3	Right to Left	15·7
	Double to Right	−24·9	Left to Right	−27·2
	Total: Double to Single	5·4	Total: hand to hand	−11·5

The figures for 'hand to hand' were obtained as follows:

Right to Left: (initial) T.E. of 1st set Single Left of Group A-b minus 1st set Single Left of Group A-a (final). The 4th sets instead of the 1st were used.

Left to Right: similar to 'Right to Left,' except that Group A-b is now subtracted from Group A-a.

The great advantage of the left hand over the right in Table III is only apparent. It is due largely to the fact that the subjects in Group A-a happened to be rather above the average in ability, and those of Group A-b a trifle below the average.

Table III shows a remarkable correspondence between the amount of transfer of skill from Double to Single, and the amount occurring on the passage from hand to hand. Each hand gains almost the same initial advantage whether it is preceded by the double-handed action or by a period of practice with the other hand. As in the case of 'Double to Single,' the initial positive transfer from hand to hand diminishes rapidly with further practice, even to the point of being converted into a definitely negative transfer in 'Left to Right.' 'Final Right to Left' would probably have shown the same negative transfer had it not been for the natural superiority of the A-a subjects mentioned above.

The similarity of the two sets of figures would lead us to suppose that exactly the same factors are transferred from the double to the single action as from hand to hand. In that case, it would seem to follow that those factors are of a general rather than a specific nature, a matter of mental attitude rather than of habits of manipulation. But the evidence on this point is not final.

Nevertheless, our results certainly justify us in concluding that there is an initial positive transfer of skill both between single- and double-handed action, and between right- and left-handed action, and that this initial advantage is rapidly lost with further practice. The final result is generally an actual disadvantage for the people who have come to one action after a previous practice-period on the other.

(*d*) *Negative experiments.*

Any attempt to explain this interference between the single and the double operations must be based on a study of the conditions under which the phenomenon does not occur. Accordingly, some of the tests already described were performed again with the following modifications: (A) With vision, instead of blindfolded. (B) With an altered mental attitude.

(A) *With vision.*

Two groups (C and D), of four subjects each, were used. Group C corresponded to Group A-b, *i.e.* it proceeded in the order: Single (Left then Right)—Double—Single (Left then Right). Group D resembled Group B-y in the order: Double—Single (Left then Right)—Double.

As a result of the eyes being left uncovered, the task became much easier. The subjects, therefore, were made to go three times round the figures for each trial. Thirty trials were given in each process, instead of forty. The results are given in Figs. 8 and 9.

First change. Group C (Fig. 8), like its 'blindfold' counterpart, A-b, shows a rise in the T.E. curve on passing from the single to the inherently more difficult double action. Unlike Group A-b, however, Group C shows no tendency to improve on the 'Double' with further practice, thus leading us to assume that the rise in T.E. just mentioned was due solely to the greater difficulty of the double task.

The changes in Group D (Fig. 9) are unequivocal. There is an unmistakable improvement in all curves upon passing from the double to the single operation. A more marked difference than that between the behaviour of Groups D and B-y (Fig. 6) in this respect it would be difficult to find. The difference can be ascribed only to the influence of vision, which seems to have robbed the process of Simultaneous Division of all its harmful effects.

Second change. The results on the return to the original process are exactly the reverse of what the blindfold experiments would lead us to expect. Instead of the old interference between Double and Single, we find, in both Groups C and D, an actual diminution of T.E. resulting from the interpolation of Double between Singles, and Singles between Doubles.

In general, we may state, the effect of Simultaneous Combination and Division upon any skilled action is harmful if vision be excluded. If vision be permitted, there is a tendency for this harmful effect to disappear. Further, without vision, there is an interference between single and double actions; with vision, there appears to be a positive transfer from one to the other.

We can explain this difference only by reference to the introspections of the subjects themselves. They are quite consistent on this point. All the subjects in these two groups recorded that, in the double operation, the eyes fluctuated rapidly between the two hands. The experimenter's observations revealed that, normally, the subjects' eyes rested about three times on each hand in the course of one side of the square. This fluctuation of the eyes, the subjects complained, brought about a corresponding fluctuation of attention, which prevented the movements of the two hands from ever becoming one action for consciousness. At no time were the two actions controlled by one mental 'fiat.'

It is not difficult to understand, therefore, why there was so little dislocation in passing between the double and single actions, and no interference between the two upon returning to the original operation. The double action is double only in the sense that two separate and independent single actions are taking place at the same time. To some extent they are synchronized, but there is none of that intimate fusion and loss of individuality which, we shall see later, is the mark of skilled double action, as performed without vision. (It is interesting to note that, from the subjective standpoint, the effect of vision on the performance was to convert it into a case of something approaching *successive* combination.)

We must assume, then, that the characteristic phenomena of combination and division, found in the previous experiments, are, in some way, connected with this fusion of two simultaneous movements into a mental and physiological unity. A further experiment was performed, therefore, in which this fusion of the constituents was voluntarily inhibited by the subjects.

(B) *With an altered mental attitude.*

Six subjects were used. They constituted a single group (E), and performed the following sequence: Double—Single (Left then Right)—Double. Group E thus corresponds to Group B-y, the only difference being that these present subjects were required to think of the two hands as performing separate and independent actions, and were instructed to suppress instantly any tendency towards a fusion of the two movements into a mental whole. The results are given in Fig. 10.

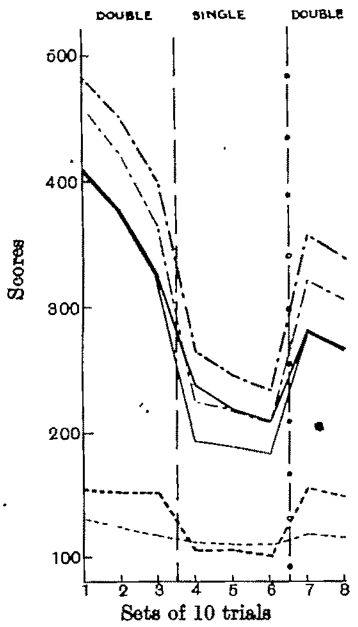


Fig. 9. Group D.
With vision.

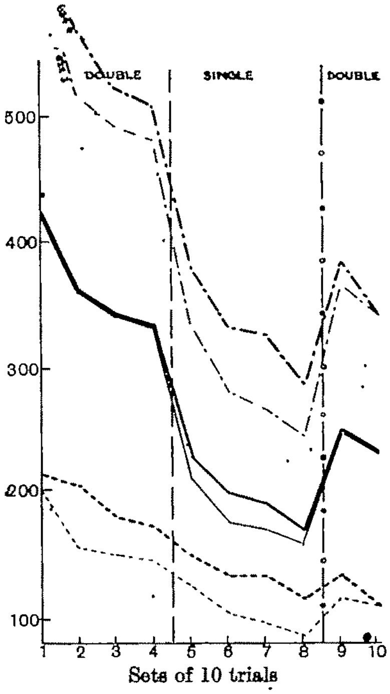


Fig. 10. Group E.
With altered mental attitude.

It can be seen at a glance that Group E is of the order of Group D (Fig. 9) rather than of Group B-y (Fig. 6). At the first change there is an improvement in the error curve, and there is no interference upon the return to the original process. By preventing the fusion of the two actions into a unified cognitive and muscular whole during the double-handed practice we have again eliminated the phenomena which, in the A and B Groups, were connected with Division and Combination.

III. CONSCIOUSNESS AND THE ACQUIREMENT OF SKILL.

But how is the 'fusion' experienced by the subject himself? Our introspective evidence on this point throws considerable light on the general relation of consciousness to the acquirement of skill. Writers on this subject are divided into two schools; first, those, who, like Pear[1], hold that concentrated attention is the secret of success in the muscular sphere, and, secondly, those who follow Koffka in asserting that the direction of consciousness "has no influence upon the efficiency of learning[2]."

Our introspective notes lead us to believe that this disagreement arises from the failure to realize that the direction and nature of consciousness varies continuously during the learning process. We have, therefore, distinguished two relatively distinct stages in the acquisition of any skilled activity, each stage offering a different problem of attention.

(a) *Learning stage.*

All subjects, without exception, reported that, in beginning the double-handed procedure, they were forced to fluctuate their attention between the two hands. The rate of fluctuation depended upon the degree of initial skill. Objectively, this was noticeable in the alternate spurts of the two hands. This fluctuation would appear to be necessary in order to enable each hand to develop a preliminary rough 'movement-pattern' of its own in the focus of consciousness. Such 'patterns' seem to be incapable of developing on the margin of consciousness. The only possible solution in such cases is a rapid alternation of the focus between the two constituents.

Speaking in terms of Spearman's "Principle of Mental Energy," we may say that each hand demands the majority of the available mental energy in order to organize its own 'pattern' sufficiently to enable it to function at all. The fluctuation of the focus of consciousness is a fairly successful compromise between the rival claims of the two hands.

Two intermediate steps were noticed by many subjects in the progress from the 'learning stage' to the 'expert stage.' The first is what we have termed the stage of 'semi-crystallization,' when the focus of consciousness, in its fluctuations, lingered on one of the hands to the relative exclusion of the other. The total action then tended to 'crystallize out' around the dominant hand. The other hand simply followed. Such an organization about one constituent was always of a temporary nature, and was broken down by an error in the neglected hand. But it seems to represent a premature effort after some kind of unity between the constituents.

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[1] *Skill in Work and Play* (London, 1924), pp. 37–45.

[2] *The Growth of Mind* (London, 1924), p. 259.

The second transition stage was noticed by only some of the subjects. When each hand had become so far automatic as to demand no longer the whole of the available mental energy, it was found that a bifocal consciousness evolved. There was no alternation of attention, and yet one had a clear focalized consciousness of each hand as more or less independent of the other.

(b) *Expert stage.*

Focality of consciousness, we have seen, is the essence of the 'learning stage.' The 'expert stage,' on the other hand, is characterized by diffusion of attention, non-focality. The state of mind is best understood by reference to extracts from subjects' introspections: "Apparently attend to nothing at all. Work best in a vague, uniform, even slightly dazed state of mind." "A blank, mechanical feeling." "Let the hands swim." "Hazy, like the feeling when you are just going to sleep." "Like the early stages of hypnosis. The general level of consciousness is low."

It was found that this state of non-focality of consciousness always accompanied the most efficient performances. In only a few of the more proficient subjects did it show any permanence. The rest experienced short periods of diffused consciousness which were rapidly brought to a close by an error in either hand. In the Vision Groups it was never reported. But there was general agreement amongst all the other groups that the acquirement of real skill in the double-handed performances was coincident with the oncome of this state of non-focality of consciousness.

This seems to fit in with a research of Spearman's in which blindfolded subjects were required to localize a tactual sensation on a passively moved limb. When attention was focused on the sensations the error was twice as large as it was with non-focalized consciousness. Spearman explains this by assuming that, in the non-focal state, more mental energy is liberated for 'unconscious articular factors.' A similar explanation might cover the present case[1].

The efficiency of the non-focalized 'low-pressure' consciousness in routine motor performances is what we might be led to expect from such extreme cases as walking and writing, which are carried on almost unconsciously, and which are liable to suffer if they become the centre of attention.

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[1] See Spearman, *Die Normaltauschungen in der Lagewahrnemung.* Also Spearman, this *Journal*, 1905, p. 287.

A further interesting stage was noted by some subjects, a stage in which there was some slight degree of focality, but in which the focus of attention rested not on any part of the action, but on some external object, such as the hum of the motor that drove the kymograph, or upon the act of whistling or counting. Such subjects reported that the pure non-focal state was the most efficient, but that the tendency to focalize was too strong to resist for long, and that, sooner than ruin the unity of the whole action by focalizing upon any constituent, they chose some external object of a fixed or rhythmic nature for the focus.

IV. SUMMARY AND CONCLUSIONS.

(1) Division of a movement-whole into its simultaneous constituents results in a loss of efficiency in the performance of those constituents.

(2) Conversely, a combination of relatively simple movements into a movement-whole results in a loss of efficiency.

(3) A still further loss accompanies the return to the original performance, after either division or combination.

(4) From (1), (2) and (3) we have concluded that constituent movements are essentially different according as they are performed in combination or in isolation.

(5) There is an initial positive transfer between the single and the double operations, but further practice converts this into a final negative transfer.

(6) There is the same amount of transfer, initial and final, from Double to Single as from hand to hand. From this we have concluded that the transfer is general rather than specific in nature.

(7) There are two main stages in the process of acquiring skill: (*a*) *Learning stage.* Characteristics: focality and fluctuation of consciousness. (*b*) *Expert stage.* Non-focality of consciousness.

(8) In so far as any factor prevents the formation of this non-focalized consciousness around a movement-group there will be no 'fusion' of the constituents, and the phenomena noted under (1), (2) and (3) will not occur.

(9) Non-focalized consciousness is the ideal to be aimed at in the acquisition of mechanical skill, but, in practice, it will often be found necessary to satisfy the almost irresistible tendency towards focalization by the introduction of some experience relatively external to the central task, to serve as the object of this focus.

(10) By deduction from the above principles it would appear that the 'whole' method of learning a muscular habit is preferable to the 'part' method, but further detailed research is necessary in order to discover at exactly what degree of complexity in the movement the disadvantages of the 'whole' method outweigh its advantages.

(*Manuscript received* 19 *June*, 1929.)

CHILDREN'S THINKING[1].

BY VICTORIA HAZLITT.

(*From the Psychological Laboratory, Bedford College, University of London.*)

I. *The theories of Piaget* (pp. 354–355).

II. *Criticism of these theories* (p. 355).

III. *Experimental investigations* (pp. 355–360).

  (*a*) *The problem of making an exception* (pp. 355–357).

  (*b*) *The problem of generalization* (pp. 357–360).

IV. *Conclusions* (pp. 360–361).

I. THE THEORIES OF PIAGET.

ONE of the most interesting and important works on child psychology that have appeared during the last few years is that of Piaget on the language and thought of the child. I propose to outline very briefly some of Piaget's views, and to give an account of work that I have been doing which bears upon them.

For Piaget the most fundamental fact about childish thinking is that it is egocentric. To this fact he ascribes a number of peculiarities which he holds differentiate qualitatively children's thinking from that of adults. He comes to his conclusion with regard to childish egocentrism from a study of children's spontaneous chatter in the classrooms of the Maison des Petits. He found that children of 3 years of age made 28 per cent. more remarks addressed to no one in particular than children of seven. He holds that the social life of the child does not begin until about 7 years of age, and that before that time, on account of his egocentrism, his thinking processes are handicapped in specific ways, the chief and most fundamental of which is his inability to see relations. For instance, such relations as that of brotherhood are misconceived by the child because he can only see things from his own point of view—even though he knows that he has a brother X, he cannot realize that X must therefore have a brother, viz. himself. Piaget shows that relations such as that of exception (cutting out an individual or group from a generalization) cannot be expressed verbally until seven or later. This inability to deal with relations leads, according to Piaget, to certain peculiarities of childish thinking, such as syncretism—the tendency to place side by side things that should have a more significant relation; transduction—the tendency to generalize from one example.

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[1] Read before the Psychology Section of the British Association in South Africa, July 1929.

II. CRITICISM OF THESE THEORIES.

It will be seen that Piaget's theory implies that the child's thinking differs fundamentally and in kind from that of the adult. What he says with regard to egocentrism and the child's lack of ability in thinking relations before the age of seven would, if taken literally, imply that children at the earlier period are scarcely capable of thinking. Now such a view runs entirely counter to common sense. If the child's thinking is really distorted in the way Piaget suggests, how comes he to master his world as well as he does in the first few years? Further, individual observations of quite young children often afford evidence of a logic and a grasp of relations wholly unexplained on Piaget's view. When we examine Piaget's work to discover what led him to his obviously false conclusions, we find two assumptions for which he gives no evidence. Indeed, he himself admits in different places that they are not justified, but still assumes them in drawing his conclusions. The first is that thinking can be identified with ability for verbal expression. The second is that the matter of thought is indifferent to the process; that if, for instance, a child cannot think a relation in regard to one subject-matter, he cannot think it in regard to another. In the study which I am going to describe, I tried to collect evidence bearing on the validity of these assumptions, and to discover whether children's thinking differs from adult thinking in the radical manner suggested by Piaget, or whether the difference is one of degree.

III. EXPERIMENTAL INVESTIGATIONS.

(a) *The problem of making an exception.*

With these ends in view I tried to invent ways of making the child perform practically some of those operations of which Piaget says that, judged by his powers of verbal expression, he is incapable. I shall deal first with the problem of making an exception. It is quite easy to verify Piaget's claim that few children under seven use spontaneously or *can* use correctly the word, 'except.' The following experiments were an attempt to get beyond this to the beginning of the child's ability to make an exception practically. With eighty-eight children ranging from three to seven years of age a Russian egg was used. It consisted of a number of differently coloured eggs shut, the smaller within the larger, into one large blue egg. The series was opened out and laid in front of the child, and he was told to put them all back except the green. All the work was individual. The youngest children tested, and dull, older children did not take up the problem at all, they simply played. Slightly older ones took up the problem and packed the eggs back, but failed to except the green, either because they did not understand the instruction, or because it dropped out of mind before they came to the stage for observing it. When they failed to carry out the instruction, although obviously trying to do so, the experimenter gave them another trial, avoiding the use of the word 'except.' "Put them all back but not the green one." Several who had previously failed succeeded to this form of instruction. Now on Piaget's view these children could not think an exception and yet, in what was for them a complicated and absorbing process, they managed to make an exception practically. Is it then legitimate to assume that the whole core of thinking lies in being able to attach the given process to the word 'except' rather than to any equivalent that the child may have?

The youngest child to solve the problem to the word 'except' was aged 3 years 4 months. When I found that very young children could solve this problem I tried them with another in which a group instead of an individual had to be excepted. The material consisted of twelve cards in different colours. Four of them were ornamented with a moon and a star. The child was told to put them all down on a tray in front of him except those that had a moon and a star. As before, the youngest merely played with the material and did not attempt to solve the problem. Another group divided the ones to be excepted from the others but applied the generalization to the exceptions. The youngest child to carry out the instructions perfectly was aged 4 years 8 months. It is obvious that this problem requires a much greater effort than the previous one, as the instruction has to be applied to a series of cases. Finally, I gave a problem that involved making an exception—although no verbal instruction to that effect was given—and then formulating the process. The material consisted of coloured and ornamented cards, mixed in with twelve black of the same size and five smaller black cards. The experimenter told the child that some of the cards were called *K*, and that he was to watch the experimenter put the pack down one at a time and to notice to which she called out the name K. The experimenter called out K as she put down each of the large black cards in rows on the child's right. The others, including the small black, she put on his left without giving them any name. All the cards were then collected and, after being shuffled, they were dealt out one at a time and the child was told to call out K to the right ones.

As with the other problem, there were some children who did not seriously attempt a solution. Others who attempted it failed to except the small blacks, and called all blacks K. In some cases these children succeeded when the whole process was repeated, in others they did not. All the children who succeeded in solving the problem were asked how they would tell someone else which were the K's. The following is typical of the conversation that would ensue with children under five.

Child.	The blacks are K.
Experimenter.	All the blacks? Yes.
	Is this small one K? No.
	Is it black? Yes.
	Then are all the blacks K? *Yes.*

These children could make the exception practically, but they could not formulate it at all. At the next stage they would formulate it in one of these ways: "The large blacks are K's, the little blacks are not"; "The blacks are K, but not the little ones." It was only bright children of seven or eight who would say, "All the blacks, except the little ones, are K." With the children who had not reached this stage and who could only express the matter in one of the alternative forms that I have mentioned above, I took a further step and gave them the sentence, "All the blacks except the little ones are K" with the word 'except' omitted, and asked them which of the three words, 'although,' 'except,' 'but not,' would make the best sense. The youngest children would agree to all the alternatives. Slightly older they favoured 'but not.' Finally, a large group of children who did not use the word 'except' spontaneously chose it unhesitatingly from the alternatives offered them.

This series of results from the success of the youngest child to except the green egg to the seven year old's spontaneous use of the word 'except,' gives some very slight idea of the enormous number of integrations that must take place before a relation which can be thought can be put into the accepted verbal form.

(b) *The problem of generalization.*

All modern writers on children's thinking comment on their errors of generalization. A great many, and Piaget is one of these, speak as though in this respect the child's thinking might be sharply contrasted with that of the adult. I have begun to collect data on how children tend to generalize. The first problem involved in generalization is that of seeing a common element in a number of things. With the youngest children I used simple concrete material. One series consisted of four small trays holding respectively:

Dog and bird.
,, pig.
,, cow.
,, sheep.

The other series held:

Matchbox and man.
,, hunter.
,, can.
,, goose.

The two series were used separately; both were used with all the children to ensure as far as possible that any failure that there might be was not due to the particular material used. The experimenter would draw the child's attention to the four trays and ask, "What is there on all the trays?" If this did not succeed, she would vary the wording, "What is the same on all the trays?" "What have all the trays got?" This last proved to be much the most intelligible form to the children. Very few children under five could pick out the common element in either of the series. The dog seemed to be much easier for them than the matchbox which, from its familiarity, apparently, fell into the background in relation to the other object on each of the trays. No child under four years solved the matchbox problem. The youngest to solve the dog problem was 3 years and 2 months. When a child failed with either problem I asked him to name the objects on each tray in turn, so that the common object would be named four times, each of the others only once. Immediately the child had named them all I would say, "What is there then on all the trays?" No child that was unable to name the common object was helped by this repetition of the names. This seems to show that their failure was not due to inattention or lack of method in their way of attacking the problems. What then was the difficulty? There seems no doubt that the realization of sameness in difference is an experience that comes more or less suddenly to the child, and that in some it does not come much before three years. Difficulties with regard to it often show themselves in the early stages of numeration, and one observer remarks on her son's wonder and excitement at two Vim tins standing side by side. He kept on drawing attention to their being alike. This was at about two years of age. If the recognition of the sameness of two things does not come much before this, to see what is the same in as many as four different things must be a very great effort to the three-year old. Further study of this problem would be interesting. The evidence to hand shows that recognition of a common element is possible for such very young children, that any disability that they may have in generalizing at the ages of which Piaget speaks cannot be due to an inability to see a common element.

Another important process in generalization is that of extending what is known to be true of certain cases to others which are not exactly the same, but which resemble them in various ways. For the study of this, material of a very simple nature and of a kind that would be equally unfamiliar to all was used. It consisted of thirty cards of different colours, some plain, others marked with gilt moons and stars in different arrangements. The child was told that two which were shown to him were called *A*. One of these was blue with a moon and star upon it, the other was yellow and similarly decorated, except that the moon and star were not in exactly the same relative positions. He was then asked to pick out all the other *A*'s from a pack, none of which was identical with the blue or the yellow as to the arrangement of the moon and star. The youngest children to attempt this picked all the cards with marks of any kind, omitting only the cards that were quite plain. Slightly older children chose only those with a moon and star, rejecting all the plain ones and all with such marks as two moons or three stars. All these children chose without any reference to the colour of the background. The youngest child to show any change on this procedure was an exceptionally intelligent boy of six, who chose only blue and yellow cards with a moon and star. The brighter children of seven and eight took into account the relative position of the moon and star, and tried to choose only those in which it was the same as in the two samples. The various bases of generalization that the children mentioned when they were questioned about their choice may be summarized in the order of their appearance with age thus (beginning with the youngest):

(1) Gilt marks of any kind on any background.
(2) Moon and star in any position on any background.
(3) Moon and star in any position on blue or yellow.
(4) Moon and star in roughly the same relative position as in sample on any background.
(5) Moon and star in roughly the same relative position as in sample on blue or yellow.

No child tested raised any difficulty with regard to the problem, although the older ones were obviously puzzled as to what to take for the basis of generalization. They were much longer than the younger children in choosing the cards. Of the whole group it is true that there was present in the mind of the child some basis for generalization. The youngest took the broadest basis, and the oldest the narrowest. Incidentally, it is interesting to note that difference in colour of background was the thing that affected choice least, and that the children who did take it into account were the oldest or the most intelligent.

IV. Conclusions.

Writing some years ago, Prof. Burt declared that he found no evidence of any specific process of thought that could not be performed by a child of seven. I should like to suggest that, from the evidence that is accumulating, it seems likely that there is no age limit in relation to the processes of thinking, beyond that imposed by lack of experience. If Piàget were right when he says that childish egocentrism makes certain processes of thought impossible before seven years of age, how could we account for the evidences of these processes at three and earlier? The truth seems to be that the child's egocentrism is largely due to his lack of experience—his world is so often made by others to foster in him the notion that he is the centre of it. His lack of experience makes him unable to see relations, and his inability to see relations makes him egocentric. It may be noted in parenthesis that Piaget's assumption that children are naturally non-social in their thinking until the age of seven is being questioned by psychologists who are working with young children. Baldwin and Stecher note social conduct and a degree of cooperation as early as two years of age in circumstances that favour and give ample opportunity for it. Very few children under five, and still fewer under three, have such opportunities, which can only come to the child who is with others at approximately the same level of development. When he is with children who are much younger or much older, the relations cannot be reciprocal, and he does not derive the benefits that he would from a social environment.

A further argument against the view that there is any radical difference between the thought processes of the child and of the adult, comes from the fact that when the adult has to think about wholly unfamilar material he makes the same mistakes as the child. He even does it with familiar material if he has not been obliged to concern himself with it previously. For instance, Piaget tells how children in accounting for the displacement of water by objects of differing size and density sometimes give the weight as the cause, and sometimes the size, without bringing the two into relation or seeing any difficulty in giving different causes for the same phenomenon. I repeated his experiment with some non-scientific adults and found exactly the same results. They were no more disturbed by the contradictions in their answers than were the children. Piaget's picture of a striking difference between adult and childish thinking is, I believe, due to an over-valuation of verbal expression as a measure of thinking, and an exaggerated view of the logicality of adult thought.

(*Manuscript received* 18 *October*, 1929.)

THE QUANTITATIVE ESTIMATION OF THE SENSATION OF COLOUR.[1]

BY T. SMITH.

(From the National Physical Laboratory.)

THE recent papers by Dr L. F. Richardson and Mr R. S. Maxwell on The Quantitative Estimation of the Sensation of Colour are an attempt to controvert William James' statement "To introspection, our feeling of pink is surely not a portion of our feeling of scarlet" by producing quantitative estimates of the relation between white, pink, and scarlet. My purpose in this note is to suggest that the enquiry they conducted was not suitably framed for the elucidation of this question, and that the results obtained may have a different significance.

Reliable views on psychological questions of this type are not readily obtained; the widespread study of physics makes it increasingly difficult to find numbers of intelligent persons able to analyse their sensations without reference to the known nature of the exciting stimulus. My own introduction to physics was sufficiently late for me to remember my surprise on learning that pink is a desaturated red. Up to that time I had thought of a relation between pink and scarlet as little as one between green and blue or between yellow and blue. To a large extent these unsophisticated views remain: browns do not in general appeal to me as dark shades of orange: lilac and mauve sensations belong to a class of their own, having no obvious connection with either red or blue. I have noticed the same thing in children: they will say for example that the marks made by certain crayons are of a quite different colour from those of the chalks themselves, though with other colours the corresponding saturation contrasts excite no comment.

It appears to me that Dr Richardson and Mr Maxwell have begged the whole question by calling for a quantitative answer. The only pertinent question is whether for each individual the 'feeling' of pink is or is not a portion of the 'feeling' of scarlet. The very fact that in conducting these experiments it was necessary for the investigators to explain that there is no correct answer suggests that for many of the subjects the two feelings are not associated. It is also important to realize that their fundamental question was put in such a way as to ensure an answer apparently unfavourable to William James. Any mark on a line connecting white and scarlet would serve this purpose.

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[1] See this *Journal*, XX, 1, 1929, and XX, 2, 1929.

This is not the occasion on which to discuss the very complex factors introduced by taking the relative distances of three marks rather than the order of a greater number of points as the basis of computation. The intention in these experiments is to induce the subject (i) mentally to estimate the number y, where

$$\frac{y}{100}=\frac{f(\gamma)-f(\alpha)}{f(\beta)-f(\alpha)},$$

and $f(\alpha), f(\beta)$ and $f(\gamma)$ represent the measures of sensations evoked by stimuli α, β, γ (the existence of some innate sensation scale being assumed), and then (ii) to guess which point of a line of given length is distant the percentage y of the whole length from one end. The process is evidently not simple. That the position of the marks were somewhat widely scattered is not surprising. If any feature calls for explanation it is the definite character of the curves, resembling hyperbolas, connecting the means given in Mr Maxwell's paper.

Pressed to compare sensations qualitatively distinct, it is natural that we should, without necessarily realizing the criterion adopted, seek for some measurable attribute which all the sensations possess in common, though in different degrees. In experiments on sight the attribute of brightness is very striking, and exists quite apart from colour. It is not difficult to make approximate estimates of relative brightness, though precise measurement in the presence of colour differences is difficult. For example, we have no difficulty in forming very fair estimates of the relative brightnesses of red, orange and green railway signal lights. It is not improbable that, in the absence of experience in colour mixture estimations, the opinions given in these experiments actually relate to estimates of relative brightness. If this be assumed, we can determine from well-established laws what the character of the observed curve should be. Let α be the brightness of the white surface, β that of the red surface, γ that of the pink when the fraction x of the total angle is red, the remainder being white. Then

$$\gamma=(1-x)\,\alpha+x\beta,.$$

and $f(\xi)$ is to be interpreted as $\log \xi$. Thus we have the theoretical relation

$$\frac{y}{100}=\frac{\log\gamma-\log\alpha}{\log\beta-\log\alpha}=\frac{\log\gamma/\alpha}{\log\beta/\alpha}=\frac{\log\,(1-x+x\beta/\alpha)}{\log\beta/\alpha}.$$

It was expected that β/α for the samples used would lie between 1/5 and 1/10. Trial with $\beta/\alpha = 0{\cdot}14$ gives the following results, with which Mr Maxwell's means are compared. All the figures are expressed as percentages.

x	y calculated	y observed (Maxwell)
0	0	0
20	9·6	10
50	28·6	27
80	59·2	58
90	75·6	76
95	86·4	91
100	100	100

The agreement of the observed means with the values calculated on the supposition that relative brightnesses are being estimated is closer than could have been expected from such a series of experiments. This comparison of course does not entitle us to infer that the estimates were in fact based on brightness comparisons, but it suffices to show that these investigations do not establish any facts tending to discredit William James' opinion.

(*Manuscript received* 18 *November*, 1929.)

THE QUANTITATIVE MENTAL ESTIMATION OF HUE, BRIGHTNESS OR SATURATION.

A REPLY TO MR T. SMITH'S CRITICISMS.

BY L. F. RICHARDSON AND R. S. MAXWELL.

THERE seems to be a fairly general agreement that the classification of 'formless visual sensations' (by which we mean uniform expanses of colour, white, grey or black) requires three independent qualities. Parsons[1] names them (1) hue = tone = Farbenton, (2) brightness = luminosity = Helligkeit, (3) saturation = purity = Sättigung. Hereinafter we shall name them hue, sensation-brightness and saturation. The unqualified word 'brightness' is confusing; for it is used by Mr Smith to denote a stimulus in some sentences, a sensation in others. We also have formerly not been sufficiently careful in this matter. The word 'tone' is ambiguous, for artists use it to mean degree of brightness or darkness, and so we shall avoid it. This classification is not quite universal, for Myers[2] uses four qualities: hue, intensity, saturation and brightness. However, we shall here follow the majority in supposing that three independent qualities are enough.

Pink, as popularly understood, differs from scarlet in being (1) a brighter sensation, (2) less saturated. It may or may not differ slightly, (3) in hue.

This statement can be made clearer by reference to water-pigments spread on white paper. On looking at a mixture of Chinese white with Messrs Winsor and Newton's alizarin scarlet, we experience a sensation that is brighter and less saturated than, and almost of the same hue as, that produced by the alizarin scarlet alone in a thick coat. The former sensation would ordinarily be called pink.

It is possible to produce a sensation which differs only in saturation from that due to a thick coat of alizarin scarlet laid on white paper. The appropriate stimulus can be arranged by adjusting a mixture of the same scarlet with Indian ink and burnt umber. But the result is not pink in any ordinary sense. It would have to be called a pinkish brown. The cover of this *Journal* is an example of pinkish brown; the cover of Parsons' *Colour-Vision* is another, but less bright and more saturated.

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[1] Sir John Herbert Parsons, *An Introduction to the Study of Colour Vision*, p. 32, Camb. Univ. Press, 1924.

[2] C. S. Myers, *A Text-Book of Experimental Psychology*, I, 71, Camb. Univ. Press, 1925.

"Our feeling of pink is not a portion of our feeling of scarlet" thus refers to sensations which differ in saturation, as well as in brightness. In testing the truth of this dictum, both sensation-brightness and saturation must be varied; they must be varied together, not independently; and the observer must be asked to make a judgment on the effect of sensation-brightness and saturation jointly. We asked the observers to judge on a scale of 'redness'; for in framing instructions popular words had to be used.

It may be said that such a procedure is a muddle; for it would be more scientific to try quantitative mental estimates of sensations when hue, sensation-brightness and saturation were varied separately one at a time. We agree, and hope that such experiments will be made. But they would not test James' dictum. That is why we deferred them.

A possible defence of James not mentioned by Mr Smith, would be to point out that we have replaced James' doublet 'pink and scarlet' by a triplet 'white, pink and scarlet.' To use a geometrical phraseology we have dealt with sensation-intervals; James may possibly have intended sensation-points.

It is generally agreed that formless visual sensations are best classified by arranging their representative points in a volume. For example, C. S. Myers[1] gave a diagram in the shape of two cones, base to base, with white at one vertex, black at the other vertex, the greys along the axis. So that sensation-brightness increases steadily along the axis, saturation increases with distance from the axis and hue depends on angular position round about the axis.

We now[2] add the further, and controversial, idea that the distances of the representative points can be so chosen that when point B lies on the straight line AC, the ratio of lengths $AB : BC$ becomes equal to the ratio of the corresponding sensation-intervals as judged by an average observer.

From the way in which the pink stimulus was made on our colour wheels, it was evident that as the energy in the light-waves increased, owing to increasing angle of the white sector, so also the distribution of that energy would spread into wave-lengths shorter than 0·6 micron.

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[1] C. S. Myers, *An Introduction to Experimental Psychology*, p. 18, Camb. Univ. Press, 1911.

[2] Apparently in agreement with Peddie, *Nature*, p. 791, Nov. 23rd, 1929.

Sensationally it appeared to our eyes that the pink was intermediate between the white and the scarlet, both in saturation and in sensation-brightness. We purposely arranged it so. Thus we think that the white, pink and scarlet as exhibited by our colour wheels lay in an axial plane of Myers' cones, and in a straight line in that plane.

Let the representative points be A for white, B for pink, C for scarlet. Let their normal projections on to the axis of the cones be A_1, B_1, C_1, and their projections on to the basal plane be A_2, B_2, C_2. Then because ABC is straight, therefore $AB : BC = A_1B_1 : B_1C_1 = A_2B_2 = B_2C_2$. If the observer judged by sensation-brightness alone (as Mr Smith alleges), he would estimate $A_1B_1 : B_1C_1$. If he judged by saturation alone, as the instruction about 'redness' may perhaps have suggested to some observers, he would estimate $A_2B_2 : B_2C_2$. If he judged his sensations without troubling to analyse them into saturation and brightness, he would estimate $AB : BC$. But all these ratios are equal. This may explain how Mr Smith's formula, derived from considerations about sensation-brightness, comes to be such an excellent fit to observations involving saturation.

Was the influence of the experimenter in our observations misdirected or excessive? We think that it was neither. In Richardson's research some eminent observers were quite willing to try the experiment; and the mean of their estimates (see Group 8) for the wheel was 44 per cent., in close agreement with the mean of 43 for all the men, and not far from the mean of 40 for all the schoolboys in Maxwell's research.

Mr Smith remarks, "their fundamental question was put in such a way as to ensure an answer apparently unfavourable to William James." The word 'ensure' is an exaggeration, for observers were at liberty to refuse to put any mark on the line, and the numbers of those who did so refuse are published. Mr Smith might truly have said 'suggest' instead of 'ensure.' But as the task of estimation requires some effort of attention, it is necessary to persuade the observer to try.

In spite of Mr Smith's criticisms, we continue to think that, by attacking James' dictum about pink and scarlet, we have cleared the way towards a more scientific study of quantitativeness in sensations of various kinds. The rare mistakes of a genius like James are apt to enslave the rest of us.

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[1] Richardson, "Quantitative Mental Estimates of Light and Colour," this *Journal*, xx, 1, 27–37.

(*Manuscript received* 30 *December*, 1929.)

THE AVERAGE VALUE FOR THE PROBABLE ERROR OF TETRAD DIFFERENCES.

BY C. SPEARMAN AND K. HOLZINGER.

IN 1925 an average value* for the probable error of all the tetrad differences in a table was given. This value, while good enough for most practical purposes, may be slightly improved by considering certain omitted products. The new formula was given as 16 A in the Appendix of *Abilities of Man*† but was derived too late for a proof to be included in the text. It is the purpose of the present paper to furnish the proof of the new approximative formula.

To obtain the new value we require the average of

$$N\sigma_t^2 = (r_{12}^2 + r_{13}^2 + r_{24}^2 + r_{34}^2) - 2\,(r_{12}r_{13}r_{23} + r_{12}r_{14}r_{24} + r_{13}r_{14}r_{34} + r_{23}r_{24}r_{34}) + 4r_{12}r_{13}r_{24}r_{34} \qquad \ldots\ldots(1).$$

I. Consider first the terms in r^2. If $\bar{r}$ denotes the mean of the correlations in the table, s^2 their variance and n the number of variables, the mean of the different terms in r^2 is $\bar{r}^2 + s^2$.

Therefore

$$\sum_1^{3C_4^n} (r_{12}^2 + r_{13}^2 + r_{24}^2 + r_{34}^2) \div 3C_4^n = 4\bar{r}^2 + 4s^2 \qquad \ldots\ldots(2).$$

II. Consider next the terms in rrr. For the whole table there are $3C_4^n$ sets of terms like $(r_{12}r_{13}r_{23} + r_{12}r_{14}r_{24} + r_{13}r_{14}r_{34} + r_{23}r_{24}r_{34})$ or a total of $12C_4^n$ terms in rrr‡. The number of different terms of the form $r_{12}r_{13}r_{23}$ is C_3^n since it is equal to the number of ways in which three subscripts may be selected from n. The number of repetitions of

$$\sum_1^{C_3^n} r_{12}r_{13}r_{23} \text{ is therefore } 3\,(n-3).$$

Therefore

$$-2\sum_1^{3C_4^n} (r_{12}r_{13}r_{23} + r_{12}r_{14}r_{24} + r_{13}r_{14}r_{34} + r_{23}r_{24}r_{34}) \div 3C_4^n = -\frac{48}{n\,(n-1)\,(n-2)} \sum_1^{C_3^n} r_{12}r_{13}r_{23} \qquad \ldots\ldots(3).$$

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* This *Journal* (2), XVI, Oct. 1925, 86. The same value is obtained by Pearson in *Biometrika*, 1927, XIX, 257.

† C. Spearman, Macmillan & Co., Ltd., London, 1927.

‡ Really there are $6C_4^n$ sets of terms. But as in every case the square of $r_{12}r_{34} - r_{23}r_{14}$ must equal that of $r_{23}r_{14} - r_{13}r_{24}$, we need only deal with $3C_4^n$ sets.

Next expanding $r_{12}r_{13}r_{23}$ we find

$$(\bar{r} + d_{12})(\bar{r} + d_{13})(\bar{r} + d_{23}) = \bar{r}^3 + \bar{r}\,(d_{12}d_{13} + d_{12}d_{23} + d_{13}d_{23}) \\ + \text{neglected terms.}$$

The number of different products like $d_{12}d_{13}$ is $3C_3^n$. Therefore

$$\sum_1^{C_3^n} r_{12}r_{13}r_{23} = C_3^n\bar{r}^3 + 3C_3^n\bar{r}\,\overline{d_{12}d_{13}} \qquad \text{......(4)},$$

where the bar indicates mean value.

The value of $\overline{d_{12}d_{13}}$ may be approximated closely by noting that

$$\overline{d_{12}d_{13}} = \overline{d_t^2} \qquad \text{......(5)},$$

where d_t denotes the deviation of the mean of a tetrad from $\bar{r}$. Also $16d_t^2 = (d_{12} + d_{13} + d_{24} + d_{34})^2$, whence,

$$\overline{d_t^2} = \tfrac{1}{4}s^2 + \tfrac{1}{2}\overline{d_{12}d_{13}} + \tfrac{1}{4}\overline{d_{12}d_{34}} \qquad \text{......(6)}.$$

The total different terms of type $d_{12}d_{13}$ for n variables is $3C_3^n$, the total terms of type $d_{12}d_{34}$ is $3C_4^n$, and the total terms of both types is $C_2^{C_2^n}$. Using Yule's* theorem we have

$$\text{Mean of both types} = -C_2^{C_2^n}\frac{2}{(n-1)(n-2)}s^2 = 3C_3^n\overline{d_{12}d_{13}} + 3C_4^n\overline{d_{12}d_{34}}$$

and
$$\overline{d_{12}d_{34}} = \frac{-2s^2}{(n-2)(n-3)} - \frac{4}{(n-3)}\overline{d_{12}d_{13}} \qquad \text{......(7)}.$$

Combining (5), (6) and (7) we obtain

$$\overline{d_{12}d_{13}}\dagger = \frac{n-4}{2(n-2)}s^2 \qquad \text{......(8)}$$

and
$$\overline{d_{12}d_{34}} = \frac{-2s^2}{(n-2)} \qquad \text{......(9)}.$$

Substituting the value of (8) in (4) and then in (3) gives

$$-2\sum_1^{3C_4^n}(r_{12}r_{13}r_{23} + r_{12}r_{14}r_{24} + r_{13}r_{14}r_{34} + r_{23}r_{24}r_{34}) \div 3C_4^n \\ = -8\bar{r}^3 - 12\bar{r}\frac{(n-4)}{(n-2)}s^2 \qquad \text{......(10)}.$$

III. Consider next the terms in $rrrr$. We may write

$$(\bar{r} + d_{12})(\bar{r} + d_{13})(\bar{r} + d_{24})(\bar{r} + d_{34}) = \bar{r}^4 + \bar{r}^2(d_{12}d_{13} + d_{12}d_{24} \\ + d_{12}d_{34} + d_{13}d_{24} + d_{13}d_{34} + d_{24}d_{34}) + \text{neglected terms.}$$

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* Yule, *Introduction to Statistics*, chap. XI, § 11.

† An *exact* value of $\overline{d_{12}d_{13}}$ is $\frac{\Sigma(\Sigma' d_{xy})^2}{n(n-1)(n-2)}\,\frac{-s^2}{n-2}$, where Σ' is the summation of d's with a variable in common. Also $\overline{d_{12}d_{34}} = \frac{2s^2}{(n-2)(n-3)} - \frac{4\Sigma(\Sigma' d_{xy})^2}{n(n-1)(n-2)(n-3)}$.

For the whole table terms like $d_{12}d_{13}$ are repeated $(n-3)$ times and terms like $d_{12}d_{34}$ are repeated twice. Therefore

$$4\sum_{1}^{3C_4^n} r_{12}r_{13}r_{24}r_{34} \div 3C_4^n = 4\bar{r}^4 + 4\bar{r}^2\left[2\overline{d_{12}d_{34}} + 4\overline{d_{12}d_{13}}\right]$$

$$= 4\left[\frac{2(n-6)}{n-2}\bar{r}^2 s^2 + \bar{r}^4\right] \quad \text{...(11),}$$

upon utilizing equations (8) and (9).

Combining the results of equations (2), (10) and (11) gives finally

$$\overline{N\sigma_t^2} = 4\left\{\bar{r}^2(1-\bar{r})^2 + \left[1 - 3\bar{r}\frac{(n-4)}{(n-2)} + 2\bar{r}^2\frac{(n-6)}{(n-2)}\right]s^2\right\} \quad \text{......(12)}$$

or $$\overline{\text{p.e.}_t} = \frac{1{\cdot}349}{\sqrt{N}}\sqrt{\bar{r}^2(1-\bar{r})^2 + \left[1 - 3\bar{r}\frac{(n-4)}{(n-2)} + 2\bar{r}^2\frac{(n-6)}{(n-2)}\right]s^2} \quad \text{......(13)*.}$$

Using the *exact* values of $\overline{d_{12}d_{13}}$ and $\overline{d_{12}d_{34}}$ of the footnote on p. 369 gives

$$\overline{\text{p.e.}_t} = \frac{1{\cdot}349}{\sqrt{N}}\sqrt{\left\{\bar{r}^2(1-\bar{r})^2 + \left[1 + \frac{6\bar{r}}{n-2} - \frac{4\bar{r}^2(n-4)}{(n-2)(n-3)}\right]s^2 - 2\frac{\Sigma(\Sigma' d_{\alpha v})^2}{n(n-1)(n-2)(n-3)}\left[3(n-3)\bar{r} - 2(n-5)\bar{r}^2\right]\right\}} \quad \text{......(14).}$$

This last value is too clumsy for ordinary use and by empirical test gives values often no better than (13) because of the neglected terms in expansions of *rrr* and *rrrr*. We therefore recommend the use of (13).

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* This is the formula given as 16 A in the *Abilities of Man* and employed in that work.

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A SUGGESTED TERM:—EUPHASIA.

BY T. H. PEAR.

(From the Department of Psychology, University of Manchester.)

IN two recent publications[1] I have tried to separate skill-in-general, defined as the integration of well-adapted responses, from ability for deliberate, adequate, verbal expression. This latter ability, from one point of view, is often a special type of skill[2]. Yet nowadays it is such an important type—indeed, perhaps unique, in that it can often serve as a substitute for most of the others, as in giving orders to skilled people—that it merits a special name.

I have explained at length why 'speech-habits' seems a highly unsuitable term. The proposed use of the word 'intellect' for this ability has aroused no enthusiasm, and the meanings attached to intellect seem hopelessly irreconcilable. Yet it is urgently necessary to distinguish skilled persons who cannot verbally describe their skill from skilled persons who can; and, not less importantly, the cultured persons who, though they cannot perform skilled actions in the arts, both fine and utilitarian, can describe and criticise some of them effectively.

On the need being explained to Prof. W. B. Anderson of the Latin Department of the University of Manchester, he has suggested 'euphasia' to indicate the ability for deliberate, adequate verbal expression. Since aphasia and dysphasia, with their useful adjectives, have justified their inclusion in the vocabulary of science, there seems little need to urge in detail the international claims of 'euphasia.' In the hope that it may help to decrease the agglutination of psychological concepts involved in the use of the word 'habit,' and assist in analysing the complex of processes denoted at present by 'verbalization,' it is here proposed, with grateful acknowledgments to Prof. Anderson.

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[1] *Fitness for Work*, pp. 55 ff., London 1928, and "Some subtler skills," this *Journal*, xx, 1929, 146 ff.

[2] Cf. McGeoch, "The acquisition of skill," *Psychological Bulletin*, xxvi (8), 457, August 1929; and Gemelli. "Recherches sur la nature de l'habilité manuelle," *Jour. de Psychologie normale et pathologique*, 1929, 3 & 4.

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THE FUNCTIONS OF THE CRITIC.

By SUSAN MILES.

Is the critic, we may ask ourselves, a teacher? Is he a touchstone? Is he either, neither, or both? He is, surely, other than the teacher, and more than the touchstone.

A teacher may show an artist how to drive his vehicle up to his medium; he then withdraws. It is Haydon's word: 'vehicle.' He tells us that in his early days he found his colours drying too quickly. He appealed to an older painter for advice. "What d'ye paint in?" asked the older painter. "Drying oil and varnish." He was told to use raw oil; and he had to ask what raw oil was. He was delighted with the information that it was oil not boiled; and he used raw oil thenceforward. "Such was at that time," he tells us, "my ignorance of vehicle."

The function of a vehicle is to save a person's time, vitality, and temper. It is an important function, and an artist's time, vitality, and temper are particularly well worth saving. A teacher may save an artist's time, vitality, and temper in the same way that Mrs Beeton may save the housewife's.

The distinction between vehicle and medium, or, better, between driving the vehicle and using the medium, needs to be made clearer. Perhaps the point may be put in this way: artists and other people too may be taught to drive a vehicle—and the same vehicle. The knowledge that raw oil keeps its moisture longer than boiled oil is, presumably, useful to the house-decorator as well as to the painter of pictures. But when the painter of pictures has got out of his vehicle, and is standing on his own feet up against his medium, he begins to be concerned with something with which the house-decorator has no concern. The artist is concerned now with his technique. His teacher can teach him how to prevent his colours from drying too quickly, but not what colours to use, or what shapes to make, in order to express his own peculiar and individual aesthetic impressions. Again, poets and other people too can be taught the normal use of language: the names of things, the structure of sentences, and so on. The poet may travel in the same cart that carries the huckster to market. The exciting time begins when he gets out of the cart and begins to tackle his own job. And it is when the poet has got down from his cart that the critic's function may begin to be exercised.

Not, of course, that there is necessarily any temporal break between the artist's concern with vehicle and his concern with medium. He may go on learning such facts as that about boiled oil long after he has begun to learn his own technique. The use of the 'vehicle' metaphor can easily be pushed too far. It might be better to make the distinction by the use of other terms, and to speak of public, as distinct from private, technique. The fact that, in practice, it may often be difficult to distinguish between driving the vehicle and using the medium (or between public technique and private technique), does not mean that the difference is unimportant from the point of view of theoretical analysis.

Neither kind of technique is the concern of the touchstone. He may be ignorant alike of vehicle and of medium. Yet, in his different way, he may be of as much use to the artist as the teacher—or more. The touchstone's function is to register his own reactions to a work of art. A nod, a shake of the head, a blank look, a raising of the brows, may be register enough in the case of some touchstones in relation to some works of art. "Mary is staring dully into the fire: this has gone flat." "Mary's eyes shine: this has come off."

Perhaps the point at issue for the artist is: "Does it matter that the dates don't fit? Or that the physiology, or the topography isn't accurate?" For the artist himself the intensity of passion pushes all inconsistencies on to another plane, so that the figures of his creation speed by unimpeded to their goal. But will these inconsistencies seem to his touchstone the mere irrelevancies that they seem to him? Better still, will he be swept on with the characters without so much as realizing that inconsistencies or inaccuracies are there? Will the touchstone be convinced, on the plane of art, by what, on the plane of fact, would be impossible?

The author as he reads aloud watches apprehensively. Will that little furrow in the brow display itself, the little pout of the lower lip, the derisive gleam in the eye? Or will there come instead the quick flush, the tremor of the sensitive lip, the sudden upthrust of the chin—and triumph?

The ideal touchstone is hard to find, and when found more precious than rubies. Some of his—more probably of her—characteristics may be enumerated. She must possess utter sincerity. A single smile of polite and meaningless affability will cut away all confidence. She must be disinterested enough to make a complete fool of herself if needs be without a quiver of hurt pride. There must be the minimum of emotional inhibitions dependent upon personal associations of which she is unaware. No muddied depths must mar the crystal of her clear-mirroring pools. That she must have sensitiveness of response goes without saying: that "trembling, delicate, snail-horn perception of beauty," as Keats has called it. But as well as sensitiveness of response there must be stability. Better stony ground and prickly thistles than a quicksand.

The pure touchstone, it must be noted, makes no attempt to help the artist mend his ways. She leaves him merely with his convictions ratified or shaken. Something is wrong, or something is right; now it is the artist's business to set the wrong right, if wrong it is: the artist's own business—and the critic's; but not the touchstone's.

Before attempting to define the functions of the critic proper, it may be worth while to say something about them which probably no one would dispute. The function of the critic is dependent upon and secondary to that of the artist. This is obvious. But though it is obvious, it has some important implications which are not so obvious. One implication is that to discuss the critic's function without having first come to some conclusion as to the artist's function would be futile. Anyone who writes explicitly about criticism must write implicitly about art. Another implication is that a profitable discussion as to the nature of criticism can take place only among people who have had artistic experience. It is a first-hand appreciation of works of art that is essential, not a wide knowledge of art-criticism. We cannot deduce the nature of criticism from the study of the work of critics. But we can, or we ought to be able to, deduce the nature of criticism from our own experience—not of criticism—but of art.

It will be well at this point to summarize in the briefest possible phrases the conception of art that underlies the conception of criticism about to be set forth. Art is the adequate expression, in a medium, of an impression which the artist has experienced as worth while in itself. If this definition of art is accepted it will be possible to define the functions of the critic in derivative terms, and to suggest that the critic's functions are, first, to decide whether or not that which purports to be a work of art is an attempted expression of an aesthetic impression, and, under certain circumstances, to describe and analyse the reactions on which his decision is based; and, second, to decide whether or not the expression, if it *has* been attempted, is adequate; and, under certain circumstances, to expound either to the artist, or to the artist's audience, the nature of the artist's success or failure.

The critic must first of all determine whether or not that which purports to be a work of art *is* an attempted work of art. Is it based on something genuine or is it spurious? Has the person purporting to have produced a work of art ever had the aesthetic impression which he purports to have expressed? If he has not, there is no work of art under consideration; and it may well be that the critic's function in relation to the counterfeit is at an end. It may—or it may not—be worth while to let the pseudo-artist know that his pretensions have been noticed; it may—or it may not—be worth while to warn an existing audience that it has been duped, or a potential audience that it may be duped. Such notifications and warnings may be classed under the heading of criticism; they may indeed be a very useful part of the critic's output. They may be useful, that is to say, to the pseudo-artist or to his public, assuming—but it is a very big assumption—that the pseudo-artist wants to be a real artist, or his public to find one. But for the critic himself there is danger ahead. The study of pseudo-art is not a good preparation for, nor a good form of relaxation from, the study of genuine art. Probably not even the greatest of critics could read, say, fifty bad novels a year without being the worse judge of a Hardy at the end of it. Each bad novel will deposit its modicum of impurity to sully the clear crystal of the touchstone. And the critic, though more than a touchstone, must be a touchstone. He cannot describe and analyse his reactions unless he has reacted.

It is the critic's first business, then, to test his reactions and to decide whether or not they are genuinely critical or merely emotional. This testing may take much time and much trouble. It may involve the repeated examination of his object on different occasions and in varying moods. It may involve the study of other works of art which may in their turn act as touchstones. A critic may be carried away by, shall we say, Miss Kaye Smith's *Green Apple Harvest*, and then may re-read *The Woodlanders* to test his first reaction. Or, again, he may test his own immediate reaction by the application to it of general principles of criticism which have been evolved, either by himself or by other critics, from the study of other works of art. This may be a very valuable part of the critic's activity. Yet it too may be perilous. It will be perilous to any critic who fails to realize that all sound critical dicta will be applicable to the uncreated masterpieces of the future, as well as to those which already exist. The applicability of a critic's canons to works of art uncreated in the critic's lifetime affords the best of all possible testimonies to that critic's worth. Failure to fit in with a canon can never be the ultimate test of a work of art. In that it does not fit may lurk the condemnation, not of the poem or of the picture, but of the canon. The canons of art were not "fixed long ago by certain inspired writers whose authority it is no longer lawful to call in question," as was once asserted by an early contributor to the *Edinburgh Review*. They have been, and they are still being, deduced from a patient study of the works of art themselves. If something comes into being which is recognized intuitively as a work of art, and which continues so to be recognized after repeated tests, then any canon of art which is irreconcilable to it must be modified. The critic, even more than anyone else, must learn to expect the unexpected.

Supposing for instance that Hardy's *Dynasts* may justifiably be classed as an epic, then, since it is intuitively recognized as a work of art, Aristotle's dictum that an epic should be written in heroic metre throughout needs reconsideration. Since the critic's function is secondary to, and dependent upon, the artist's, it would be absurd to say: "Aristotle was a great critic, therefore *The Dynasts* cannot be a great epic." It is proper to say: "*The Dynasts* is a great epic, therefore Aristotle, though a great critic, erred in maintaining that an epic must be written in heroic metre throughout." But facts have never given the lie—nor are they likely ever to give the lie—to his assertion that every work of art should have a beginning, a middle, and an end. We shall do well to apply the one canon while refusing to apply the other.

When a work of pseudo-art is so much like a genuine work of art as to deceive even careful students, it may be very well worth while for a critic to spend time and trouble in analysing his own reactions and helping other people to see through the deception. When the spurious quality of the work is obvious, to spend time and trouble upon it is undesirable; the critic's time had clearly better be spent on something else.

Let us now pass on to consider the critic's second function, and consider him no longer as concerned in determining whether or not there has been any aesthetic impression to express, but in determining rather whether or not the expression of a genuine aesthetic impression is adequate, and in what respects it is adequate or inadequate. We now find ourselves faced by a very difficult question. Presumably the critic is aware of the artist's impression only through his attempt at expression; but if the attempt at expression has failed, how does the critic know what it was that the critic has failed to express? What is it that he puts over against the work of art with which to compare it?

There is an answer which at first sight, but at first sight only, may appear to be adequate. The expression, it is urged, has only partially failed. In some ways it must have succeeded, or else the critic would not be able to assert that the artist's aesthetic impression had taken place. The work of art is inadequate in the same way in which a torso is inadequate. Bits of the whole are lacking, but enough are there to enable us to infer the missing bits. It is merely a case, it is urged, of Prof. Stout's 'Relative Suggestion,' or of Prof. Spearman's 'Eduction of Correlates.' It is a case of filling up the gaps in a discontinuous series. There are enough dots there for the critic to be able to fill in the dots that are not there but that would have been there had the artist been successful.

The first part of this argument is certainly sound. It is true that the expression must have partially succeeded. What is unsatisfactory in the proffered solution is that, if there are enough dots there to enable the spectator to fill in the rest and so get his continuous series, then the attempt at expression has not failed. The work of art is adequate. "It is more blessed to guess than to be told." If there had been more dots and less guessing the work of art would have been less, and not more, successful.

No, we are still faced by the very difficult question: how does the critic know what the artist was trying to express but has not expressed? How does the critic get into touch with the artist's aesthetic impression otherwise than through the artist's expression? I think that we are driven to the conclusion that the critic is himself an artist, and that by some blessed coincidence he has himself experienced an aesthetic impression similar enough to that experienced by the other artist for him to know that it ought to have been expressed thus, or thus. (It is important to note that we are considering the case of the critic faced by an inadequate work of art, not of the mere spectator appreciating an adequate one. The two situations are very different.)

Two objections may be raised to the conclusion that the constructive critic is an artist. Great artists, it may be urged, often make futile criticisms of other artists' work, and valuable criticism is often given by critics who have themselves produced no great works of art.

These statements are true in themselves, yet they do not disprove the conclusion against which they are aimed. They may be met by the consideration that most great artists are too much occupied with their own creations to spare time for constructive criticism of other people. If pressed to give criticism when they are itching to do their own work, they may, either out of deliberate perversity, or as a defence mechanism, say some very foolish or some very spiteful things. Even when they want to be useful, and fair, and constructive, they may be so much obsessed by their own creations that they cannot see round them. They squint, or have blind spots. Probably every great artist would be a great critic if he were not obsessed by his own creations; but every great artist is apt to be obsessed by his own creations—except when he is exhausted and so unfit to give constructive criticism. Hence the extreme rarity of good constructive critics! Hence too the unsystematic nature of the best criticism. It seems to be wrung out of the great artists half against their will. Or, when it is given ungrudgingly (as, say, by Charles Lamb), it leaves one with the impression that the critic might have been an even greater artist than he was if he had been a less acute and subtle critic. Lamb was essentially a generous man. Had he been less generous, might he have been as great a poet as Wordsworth? Had Wordsworth been less grudging, less egocentric, might he have been as great a critic as Lamb? After all, was it not Wordsworth who said of Blake, at a time when Blake's genius was barely recognized: "There is something in the madness of this man that interests me more than the sanity of Scott?" Wordsworth did not want to read other men's books. He feared them perhaps. If Wordsworth had kindled to Shakespeare and Marlowe as Lamb did, would he have written about them instead of writing *Tintern Abbey*? We cannot tell; the case is, in the nature of things, incapable of proof.

But the difficulty of finding any other satisfactory solution of the question we have been considering seems to afford a strong presupposition for the belief that the greater the artist the greater the critic—provided he puts himself wholeheartedly into his criticism. It is, however, only under exceptional circumstances that the great artist will put himself into the work of criticizing. Of criticizing other people's work, that is to say. He is, of course, continually criticizing his own: a fact which is often lost sight of, but which goes some way towards the support of the contention here put forward. The truth may, perhaps, be expressed in this way: all great artists would be great critics if they gave their minds to it, and all great critics are, in flashes, great artists.

The critic and the artist have, perhaps, something in common with the two dogs of Mr Liam O'Flaherty's study. The greyhound was some years younger than the black dog. His coat was glossy as silk, and his snout was so long and pointed that he could lick milk from the bottom of a tumbler without touching its sides. Almost without moving a muscle he could jump from a standing position and go clean over his master's head without grazing it. But while the black dog methodically nosed along a rabbit's trail, the greyhound rushed about furiously, jumping stone walls with his head in the air. When the black dog started a rabbit, the greyhound outdistanced him in a few strides, but was unable to catch the rabbit because of his own terrific speed, and the rocky ground which cut his delicate paws and made him stumble. And often the greyhound turned a rabbit into the black dog's mouth; but he seldom caught a rabbit for himself.

Black dog and greyhound: both chase rabbits. Poet and critic: both pursue the same elusive prey. Greyhound critic with his sudden swift onrush speeds past blackdog artist, and turns the rabbit beauty into black dog's jaws. O'Flaherty's black dog did not love the greyhound, although the greyhound's speed and delicacy would sometimes help him catch a rabbit he would otherwise have lost. Nor does the artist always love the critic, even when the critic offers him sound and constructive criticism.

And is the critic the artist's foe? The trend of our argument has raised the question. If the critic is an artist who makes criticisms instead of art, is not criticism art's enemy? The greyhound, if he kept off the black dog's preserves, would start his own rabbits, and learn to catch them? Perhaps. And perhaps in an ideal universe there would be no place for criticism, however constructive. Art and art appreciation would form a whole completely rounded. Perhaps it is a good thing, even now, to work for the elimination of the critic and the independence of the audience. (For the independence of the artist there is little need to work—because of the natural intransigence of the creature.) Criticism is not, perhaps, of surpassingly great intrinsic value. And yet—as things are, in an unideal universe—we may be thankful for the criticism that is wrung from the artists. Probably the less professional criticism—as a thing apart, put forward by the critic who does not himself create—the better.

But the question has become that of the antagonism, if any, between art and criticism. To get back to the concrete. Is there a natural enmity, not between art and criticism, but between the artist and the critic? It would not be surprising to find such enmity; and that not primarily because of the self-centredness so common to our kind—"a poor thing, but mine own" seeming so often better than a good thing but another's—for in great art we do, surely, get beyond mere personal possessiveness. We would, probably, save our own child, even though he had an intelligence quotient of forty, if it were a choice between saving him and drowning Plato. But would Keats have saved *Endymion* at the expense of *King Lear*? It is hard to believe it. And even if he could not have brought himself to cast away his first-born, it would not have been the artist in him that flinched, but the mother—so often there along with the artist, but other than and often in conflict with him—the mother in Keats, who couldn't bear that this youngster should perish.

No, it is not mere self-centredness on which the enmity is based, but upon fear. Fear lest the great artist that the great critic is should swallow up the great artist that the artist *manqué* might be. No two men can have identical impressions; hence, however right the critic's contribution may be as an expression of the impression he has had, yet it cannot be quite right as an expression of the original artist's impression. The critic's contribution is towards a new work of art, greater than any that the fumbling artist could, at that time, have made by himself, but—however slightly—different in kind from the work of art that would have been made if the first artist could have made one by himself. And it may well be this fact that explains the stubbornness of young artists in the face of even the most generous constructive criticism.

Assuming then that we are justified in supposing that a critic is a constructive critic only in so far as he is an artist—albeit an artist fleetingly and fragmentarily—the wonder of the critic's faculty of seizing upon the artist's unexpressed impression merges in the greater wonder of the artist's faculty itself. With that, explicitly, we are not here concerned.

It is time now that we touched upon the critic's lesser functions in relation to the artist's audience: for the critic has much to do in addition to his work in relation to the artist himself. These lesser functions do not in themselves need an artist to carry them out. At its simplest, the critic's function may be little more than a gesture. He is the finger pointing; the voice that cries: "Look!" This is the anthologist's task; yet even the anthologist is a critic, since the very act of singling out that at which to point involves a critical discrimination. The editor too may be a critic, and that apart from any commentary. He too is choosing, and by his act asserting the value of that towards which he directs attention.

Nor must we fail to mention the function of the parodist, for parody, as Sir Owen Seaman has told us—and shown us—is a department of pure criticism. Not, of course, that form of parody which is the outcome of mere high spirits; there is no room for facetiousness in critical parody such as that of Mr Max Beerbohm or of J. K. Stephen. The work of the critical parodist is twofold: he may make a genuine artist realize what might happen to his work if he relaxed his own criticism; and he may show up the mannerisms, affectations, and pretensions of a pseudo-artist with a reputation. In some cases criticism can take no better form than parody: above all in the case of platitude masquerading as profundity, or sentimentality as pathos. Parody at its best may be described as condensed criticism; a really good parody may concentrate into half a dozen lines a critic's judgment, not only as to a writer's profundity—or his lack of it—but also as to his psychology, his phrasing, and his rhythms.

It may be objected that such criticism is necessarily destructive. That is true. But although destructive, such criticism is by no means necessarily condemnatory. Indeed, to be subtly parodied by a good critic is in itself a commendation. No good critic is likely to spend much time in parodying either a consistently bad or a mediocre writer. For to parody is not a quick and easy form of criticism. It takes pains as well as skill to catch the phrasing and the rhythm and the substance, and to set them free again with just those additions that will make them absurd without making them unrecognizable.

A few artists have been detached enough to be able to parody themselves—deliberately. It is as much as we can expect from most if they can succeed in deriving profit and amusement from other people's parodies of their work. As a matter of fact few artists are as detached as Henley, who is said to have remarked that he might have written Sir Owen Seaman's "Out of the large limbed night" himself—when he was drunk. The qualification, by the way, is significant, though it would have been better if Henley had said "after a few glasses of champagne" rather than "drunk": just enough to break down the barriers of self-criticism. But champagne there must be. To produce an imitation that might have been an original written after a too heavy high tea is no achievement. That Scott included Goldsmith's parody of Swift in his edition of Swift's works under the impression that Swift wrote it is no less a criticism of Goldsmith than of Scott.

Closely allied to parody is the mock panegyric, such as Edward Thomas' ironic eulogy of Ella Wheeler Wilcox. Again, there is the criticism that lies behind the interweaving of quotations such as Mr Leonard Woolf's deft mingling of Mr Middleton Murry's phrases with those of Mr Pecksniff. And there is Mr Desmond Macarthy's neat juxtaposition of Miss Stein's aphasic sentences and morsels from a typist's book of exercises.

Criticism of this type may be described as demonstrative criticism. Mr Christopher Stone, in his little book in the Art and Craft of Letters' series, alludes to it more vividly as criticism of the "Sister Mary walks like this" type. It needs to be done well to be tolerable, but when it is done well it is well worth the difficult doing. Andrew Lang, for instance, in his *Brahma*, has contributed a genuine piece of criticism, not only of Emerson's method and phrasing, but of the whole school of that idealistic philosophy which delights in "setting things up over against one another and bidding them proclaim their mutual identity."

I am the batsman, and the bat;
I am the bowler, and the ball,
The umpire, the pavilion cat,
The roller, pitch, and stumps, and all.

It is tempting to quote more examples of such criticism, but, instead, let us turn for a few moments in conclusion to the consideration of some suggestions thrown out by the late Katherine Mansfield. "Style," she once said, "is speaking to the back of the room." The saying shows her customary insight. Style is just that: being audible without straining either your voice or your hearer's ears, and without shouting. And the critic, who must himself have good hearing, is the man who sits in the back row, and determines, if the speaker is not heard, whether it is because he is dropping his voice or because the audience is deaf. Note that it is not the critic's business to be a megaphone. "Sa fonction est de dire la vérité, mais non pas de la faire croire," as Rousseau said. It is the propagandist, not the critic, who hammers in his own or other people's points. But the critic may well put forward a guess of his own, when the speaker has finished, to fill in a gap in a good speech which has been mumbled here and there. And he may well throw out a suggestion as to the choice of a building. In the Albert Hall a speaker *must* strain his voice and do outrage to the senses of the people in the front rows—unless he is to be inaudible in the gallery. It is part of the critic's function to point out this fact to anyone whose voice has qualities worth saving. To point out the fact and, perhaps, to help him find a building that will give the right range for his voice: the Queen's Hall, it may be, or the Poetry Bookshop—or a powder closet. Sometimes it may be the critic's business to invite a few friends to his own study and beckon the speaker to talk to them there.

In such terms as these might Katherine Mansfield have extended her metaphor to include not only style but the criticism of style. Instead she made another suggestion. "How," she asked, "is one to test the range of one's voice?" and her answer was that one should find a very simple friend and experiment upon him. Yes, Katherine Mansfield's friend is, perhaps, what has been called the touchstone. But there is a problem here. "How simple is the friend to be?" And the best answer seems: "He should be just as simple as the artist himself, but no simpler."

Indeed, the ideal critic of an artist's work is the artist himself stripped imaginatively of all prior knowledge of what he is trying to express. The artist's style must be grounded in utter honesty. He must speak to an audience no more acute and no less acute than himself. His test must be: "If I had been in the audience should I have needed that gesture, that inflection, that emphasis?" If not, it must go. "Should I have followed that ellipsis, have supplied the implications of that phrase?" If not, something is lacking and must be added. It is a question of how many dots would the artist himself have needed to find the continuity of the series and make the curve. The difficulty is, of course, for the artist to strip himself in imagination of that which is there in fact. His knowledge *is* there; it is hard for him to get away from it. Hence the value to the artist of someone else who does not know, but who is willing to try to find out.

The nearer the critic approximates in intelligence and in sensibility to the artist the more use he will be to him. In intelligence and in sensibility, yes; but not in experience. If the artist and the critic share a great deal of their past histories it may well happen that something may be intelligible or significant to the critic in virtue of mere personal accidents. If they have much in common in their lives, it may be specially difficult for the critic to distinguish between genuine art and mere aesthetic reproduction; and a great many more so-called works of art are, as a matter of fact, aesthetic reproductions than is often realized. They aim at producing in the spectator, not an artistic reaction to a work of art, but an echo of an aesthetic experience. They take him back to a familiar haven. Mr Hilaire Belloc has composed a little poem to serve as an illustration of this point, putting in 'Tumty' so as not to offend any school or river; fill it in as you will.

Ah, years ago, but I once was there
And I wish I was there again,
By Tumty River, and Tumty Weir,
Along with the Tumty men.

The point may perhaps be made clearer in this way. Consider the following detached phrases and give yourselves up to the imagery they suggest.

Sun-warmed strawberries, sheltered by dewy leaves.
The burr of a cider-press in a Brittany farm-yard.
The rhythmic creak of rooks' wings above a ploughed field at dusk.
Buds on a lime tree seen against sooty roofs in early May.
The thin thread of a new-born lamb's complaint.
The swish of milk in the pail and the smell of a well-kept byre.

Now, there is no *art* in these phrases. They stir you, if they stir you at all, to the renewal of past aesthetic activity, or perhaps to an emotional activity that is not aesthetic. But it is pitifully easy to string such phrases together in a well-marked rhythm ("the kind of lilt which you get from an engine wheel with a flat in it bumping down an incline"—to quote Mr Belloc again) and to delude yourself and others into thinking that you are an artist.

Near Folly Farm in April,
    Catkins, red-tipped are found.
And while the thrushes gay trill,
    Pale cowslips star the ground;
        And the wild strawberry bud,
        (And God's own mud)
    On God's own earth abound.

Near Folly Farm the Cotswold Hills
    Climb up to greet God's heaven.
Ah, their clay it is streaked with a hundred rills,
    And the stars on their summit are seven!
        And the ruts in the lane
        Lie deep-scarred as with pain
    By the carts that the farm-lads have driven.

In Folly Farm we Georgians stay,
    Beneath the wind-swept trees.
A score of us have found the way
    To fill our rhymes with breeze;
        For God's thinnest stuff,
        If it's fresh-air enough,
    Gets the public we're out to please.

To have stayed at Folly Farm together is not the best bond for artist and critic. Indeed, the most trustworthy critic will probably be a stranger. He must be a kinsman, if you will, but a kinsman from a far country, who sees our familiar streets with new eyes, and listens to the familiar rumble of our traffic with new and wondering ears. And yet a kinsman—who finds himself at home in a fair city. It is a case of Plato's philosopher, perhaps; of the philosopher, or the just man, who recognizes goodness at sight, and feels at home with it. The good critic recognizes good art, knows it, feels at home with it. The city of art is a free city needing no shibboleths to gain admittance. He that hath feet to bear him thither, let him come. And the rare and perfect critic speeds straight to the innermost temple. And there the artist waits for him in silence, knowing that if his kinsman finds his way without a stumble, or a detour, the city is well founded and well planned. His coming brings to both a Felicity comparable only to that of those who dwell in, or find their way to, that city of which Plato spoke, whose pattern is laid up in heaven for him who wishes to behold it.

> It is a good thing to be happy alone; it is better to be happy in company.... Nothing can be our happiness but that alone which we rightly apprehend....We are more happy in communication than in enjoyment, but only that communication is enjoyment: as indeed what we give we best receive. For the joy of communicating and the joy of receiving maketh perfect happiness.

So says Traherne, and with his words we may conclude, for what is the exercise of the critic but to receive and to communicate that which the artist has himself communicated and received?

(*Manuscript received* 15 *March*, 1928.)

PUBLICATIONS RECENTLY RECEIVED.

Brain Mechanisms and Intelligence: A Quantitative Study of Injuries to the Brain. By K. S. LASHLEY. Foreword by H. M. ADLER. 1929. Pp. xiv + 186. 11 Plates. University of Chicago Press. $ 3.00.

This book is a summary of Prof. Lashley's recent work on the effects of injuries to the brain in the rat, and a discussion of the bearing of the results on theories of cerebral function, intelligence, and learning. The main experimental results described in the book are as follows:

Destruction of any part of the cerebrum leads to a reduction of the rate at which a rat learns a given maze, and the amount of the reduction is roughly proportional to the amount of tissue destroyed and independent of the locus of the injury. The performance of maze habits formed before the lesion is produced, and the retention of maze habits formed afterwards, are affected in a similar way. An injury of a given extent produces a relatively larger effect on complicated maze habits than on simple ones. There is no detectable qualitative difference in the maze behaviour of rats with injuries to different parts of the cortex; but in all cases the operated animals show a "general inadequacy in adaptive behaviour" comparable with general dementia in man.

The results obtained from experiments on brightness discrimination and on the solution of a 'double-platform' problem box were somewhat different. In these cases it is found that the removal of any quantity of the cerebrum up to about 50 per cent. of the whole has little or no effect on the ability to acquire the habit. But if the habit is formed in an intact animal, removal of the occipital lobes destroys the brightness discrimination habit, and removal of the frontal lobes destroys the problem box habit. In either case the habit can then be re-learnt at a rate about equal to that of the original learning, and it does not then become localized in any particular cortical area. Lashley considers that the difference between these results and those for maze learning is probably due to the greater complexity of the maze habits.

In other experiments it was found that cutting various association tracts in the cortex had little or no observable effect on behaviour.

The difficulty of reconciling these results with current theories of cerebral localization and habit formation is obvious. Lashley argues that his results are quite inconsistent with any of the theories which attempt to explain learning in terms of alterations in synaptic resistance. His own tentative theory of cortical functioning is in terms of physiological gradients rather than nerve impulses. There is a good deal of evidence that in higher animals the localization of function in the cortex is much better defined than in the rat; but, in a critical review of some of this evidence, Lashley argues that even in the highest animals localization is not nearly so strict as is usually supposed.

Lashley's results seem likely to be of revolutionary importance, and all those interested in cerebral physiology will welcome this concise account of a large section of his work.

Biological Principles. By J. H. WOODGER. London: Kegan Paul (International Library of Psychology, Philosophy & Scientific Method). 1929. Pp. xii + 498. 21*s.* net.

This is a deeply interesting book though it is somewhat unnecessarily long. After a general introduction which deals with the historical growth of biological thought and methods, the author discusses at length the data of natural science and the principles of systematization of scientific knowledge. Naturally this section is largely philosophical in bent. Phenomenalism and its alternatives are considered in the light of recent controversies, the categories of substance and causation are passed in critical review, and an attempt is made to say what are the postulates and demands of natural science and what is contributed by subjective factors. Problems of biological knowledge are next considered. Here the relation of vitalism and mechanism plays a large part, and other antitheses dealt with are those of structure and function, organism and environment, preformation and epigenesis in individual and racial development, teleology and causation, and mind and body. On the whole the main conclusion is that the sound development of biological science depends less upon getting biologists who can apply exact experimental method than upon producing biologists who can *think*.

Physiology of the Central Nervous System and Special Senses. By N. J. VAZIFDAR. Calcutta: Thacker, Spink and Co. 1929. Fifth edition. Pp. vii + 301.

This book is a carefully arranged epitome of the main facts of the physiology of the central nervous system and special senses. The fact that it has reached its fifth edition shows that it serves a useful purpose in giving so much information in such a small compass. There are thirty diagrams, but this is hardly an adequate number, and some of them are not very clear. There are a large number of misprints; and, especially in the chapters on the special senses, a number of inaccurate and obscure statements. For instance, it is stated on p. 294 that "the velocity being proportional to the intensity of the sound, loud sounds travel more rapidly than feeble ones"; and on p. 276 the Purkinje phenomenon is described as follows, "The effects of change in intensity of light on the colours is rendered obvious by the fact that when the intensity of light is diminished, the brightness of the spectrum is shifted from the yellow to the green, and a coloured object appears colourless," no further explanation being given. Faults such as these could easily be corrected if the text were revised in a new edition; and if this were done, and more diagrams were added, the usefulness of the book would be very much increased.

An Introduction to the Study of the Nervous System. By E. E. HEWER and G. M. SANDES. Foreword by Prof. Winifred Cullis. London: William Heinemann. 1929. Pp. xi + 104. 21*s*. net.

The title of this book is misleading, since the authors have not attempted to write a general account of the anatomy or physiology of the nervous system. As is explained in the Preface, "The book...is intended to stress particularly the points which we, as teachers, find are not sufficiently clearly expressed elsewhere." "Minute structure and function have been considered together, but no description of gross structure has been given, as this can be found so admirably expressed in many modern text-books."

The tracts in the spinal cord, the deep connections of the cranial nerves, and the connections of the lower brain centres are described at some length, and illustrated by very clear diagrams. Nerve cells and fibres, the cerebro-spinal fluid, the autonomic nervous system, reflex action, co-ordination of movement, and localization of function in the cerebral cortex are treated more briefly; but in all cases a great deal of information is given in a condensed form, and the diagrams are always admirable.

Psychologists interested in the localization of function in the nervous system, as well as medical students, will find this a very valuable supplement to other works of reference.

Contributions to Psychiatry, Neurology and Sociology. Dedicated to the late Sir FREDERICK MOTT. Edited by J. R. LORD. London: H. K. Lewis. 1929. Pp. xii + 401. 21*s*. net.

It is impossible, within the limits of book notices published in this *Journal*, to do more than call the attention of all of its readers who are interested in psychiatrical problems to the publication of this magnificent work. The book, which is the work of colleagues of the late Sir Frederick Mott, is published by the Mott Memorial Committee for whom Dr Lord has acted as a most efficient editor. It is right and natural that most of the studies should be concerned with neurological and anatomical questions. But all of these are in first-rate hands, and there are in addition many striking theoretical and clinical studies of functional, mental and nervous disorders. The book deserves and will no doubt receive a ready welcome from a varied scientific public. No better memorial of a life devoted to scientific investigation in psychopathology and allied fields could have been devised.

An Historical Introduction to Modern Psychology. By GARDNER MURPHY, Ph.D. London: Kegan Paul. 1929. Pp. xvii + 470. 21*s*. net.

A more satisfactory addition to the International Library of Psychology, Philosophy and Scientific Method than a history of the development of experimental psychology could not be imagined. There will be many students of psychology who will be extremely glad to have this volume at their disposal. Dr Murphy has successfully selected most of the contributions which ought to figure in his study, though possibly he has laid more stress than is justified on the works of American authors such as Woodworth, to the neglect of Englishmen like Rivers and Myers. Apart from this there are few serious criticisms: Dr Murphy writes a more pleasant style, and is clear and intelligible under the worst of circumstances.

The book falls naturally into three parts; the history of the pre-experimental period, that of the early stages of laboratory psychology, and the final account of modern views and problems. In the first part attention is drawn to the separation by the growth of learning, of those aspects of mental science which are susceptible of experimental treatment, away from original philosophical and logical fields. This development was mainly due to the English critical philosophy and its subsequent culmination in the associationism of the nineteenth century. When this associationism waned after reaching its climax new impulses were received from the work of biologists, and from physiologists in particular. Modern psychology is almost founded upon a combination of the analytic and introspective method, with the objective attitude of the experimentalists. McDougall has emphasized this fact already. Perhaps in this volume Dr Murphy is inclined to over-emphasize the importance of the external or physiological approach. As he is writing a history of the development of experimental psychology this is necessary; but as he calls it a history of modern psychology, his attitude is misleading. Behaviourism has shown by its failure that psychology will not advance by the rigid exclusion of the one aspect; both are necessary and essentially complementary to each other. Dr Murphy hardly seems fully to understand the point of view of observational psychology that this is not the same as, or even an unscientific version of, the truly experimental method: there are some aspects of the subject that will never lend themselves to laboratory treatment. The all round psychologist will always have to be a field naturalist in some degree. The psychological field naturalist ought to be able to claim a distinguished place among scientific workers. Dr Kluger's account of the modern movements in Germany, an appendix to the history, is disappointing.

The Psychological Register. Edited by CARL MURCHISON and others. Worcester, Massachusetts: Clark University Press (London: Humphrey Milford). 1929. Pp. ix + 580. 27*s*. net.

This is a *Who's Who* for psychologists of the world. It gives date of birth, past and present official position, honours and publications of accredited psychological experts. All things considered it is a remarkably complete work of reference. Many of the lists of publications are nothing short of stupendous. There are doubtless some omissions and some inaccuracies, but Prof. Murchison has carried out a most difficult task with conspicuous success, and has produced a work that every psychological department must regard as a necessary possession. The editor promises to keep the work up to date by frequent revision.

Manual of Psychology. By G. F. STOUT, M.A., LL.D. Univ. Tutorial Press. Fourth edition. Pp. xix + 680. 12*s*. 6*d*. net.

This fourth edition of Prof. Stout's *Manual*, revised by the author and Mr C. A. Mace, contains some changes. The most important of these are the following: (1) Prof. Stout's general theory of the relation of thought to sentience with its application to the problems of perception has been re-stated and amplified. This has involved a radical revision of parts of Book III, Part II, dealing with the growth of our perception of the external world. (2) A number of amplifications have been introduced into those parts of Prof. Stout's psychological theory which have a bearing on the problems of the 'Philosophy of Mind.' These put more clearly his rejection of the views of the Atomistic school, and demonstrate certain similarities which exist between his views and those of the Gestalt school.

The Mechanics of the Life of the Soul. An Introduction to Physiological Psychology. By N. A. KABANOW. Moscow. 1928. Pp. 1–160.

This is a popular book, treating psychology mainly from the point of view of conditioned reflexes. In the first part of the book the connection between nerve physiology, brain anatomy and the simpler mental activities is quite ably worked out, but the greater part of the book is taken up by a discussion of emotions, thought, will, etc., purely from the point of view of conditioned reflexes and association and in a completely dogmatic manner. The impression of dogmatism is increased by the fact that throughout the book not a single author is named, nor is any evidence given. The problem of consciousness is not touched.

Creative Imagination. By JUNE E. DOWNEY. London: Kegan Paul. 1929. Pp. viii + 228. 10*s*. 6*d*.

Prof. Downey takes the problems of the descriptive psychology of literature, and treats them in a popular manner which is easily understandable. It is a disappointment only to the psychologist that she does not attempt a theoretical account of the part played by the functions that she analyses. She does this neither for the creative nor for the interpretative imagination, with which she is more concerned in fact. The book is merely an account of the general character of the interpretative imagination as it appears among differently constituted persons in the sphere of literary appreciation; it is not really a discussion of the creative aspects of imagination at all. It is a good account for the general reader, but much less so the specialist.

The Future of an Illusion. By SIGMUND FREUD. The Hogarth Press. 1928. Pp. 98. 6*s*.

This is the fifteenth volume of the International Psycho-Analytic Library, edited by Ernest Jones. In it Freud reviews briefly what he considers to be the psychology of the evolution of religion, taking each aspect in a fresh chapter. It is his opinion that religion, which is an illusion, can never provide the satisfactions fully that are expected of it; and that, science is bound to be substituted for it in the future, if reason is to gain control over instinct. Culture and civilization place inhibitions of a serious character on the natural tendencies of man (towards incest and to murder), and so bring deprivation to their members. Religion excuses culture for its misdeeds in these directions, provides a series of substitute satisfactions, and gives such a soothing account of the meaning of existence that all our natural terrors are relieved and our problems solved for us. We believe religious doctrines simply because our forefathers have done so for centuries, and because of our tendency to adhere to that which is pleasing and to behave 'as if' it is true. But the development of mankind is such that reason inevitably will come to govern instinct: that it does not fully now does not show that it will not some day: and then the illusion of religion will be superseded by a science which is true. Freud is the bringer of this new light, and Psycho-Analysis his torch.

Die Methoden der pädagogischen Psychologie. Von W. J. RUTTMANN. Halle a. S.: Carl Marhold. 1930. xii + 487 S. 19.75 R.M.

This first volume of a new general treatise on educational psychology covers a vast field with very considerable success. Prof. Ruttmann has really attempted a comprehensive survey of the whole of psychology, so far as current methods are concerned, in its bearing upon educational problems. The material is very clearly arranged. A brief introduction leads to a statement, with some illustration of discussion of the main division of psychology: (*a*) general, (*b*) individual, (*c*) genetic, (*d*) social, and (*e*) border line topics, with particular reference to the physiology of the central nervous system. Next, different methods are discussed and illustrated in some detail. Special sections deal with: experimental method; the method of 'exploration,' which includes the application of tests; the introspective method, the statistical method, and the 'personal' method, which includes a study of character, temperament, 'types' and the like. It is inevitable that a number of the topics considered should be dealt with in a somewhat scrappy manner, but a prodigious amount of information is imparted. The book is enlivened by 158 illustrations, and every section has a long list of references, some of which are annotated.

Research in the Social Sciences. Edited by WILSON GEE. New York: The MacMillan Company. 1929. Pp. x + 305. 8*s*. 6*d*. net.

This book contains nine lectures developed before the Institute for Research in the Social Sciences at the University of Virginia, early in the autumn of 1926. The subjects and lecturers were as follows: Sociology, by R. E. Park; Economics, by A. A. Young; Anthropology, by Clark Wissler; Statistics, by R. E. Chaddock; Psychology, by R. S. Woodworth; Jurisprudence, by Roscoe Pound; History, by A. M. Schlesinger; Philosophy, by John Dewey; Political Science, by C. A. Beard. As a contribution to a definition of varying objectives and methods the volume is one of considerable interest.

The Growth of Reason: A Study of the Rôle of Verbal Activity in the Growth of the Structure of the Human Mind. By FRANK LORIMER. London: Kegan Paul (International Library of Psychology, Philosophy and Scientific Method). 1929. Pp. xii + 230. 18*s.* 6*d.* net.

The main value and interest of this book are that scattered throughout its pages are numerous first-hand records of the verbal activity of young children. The author tries to show how verbal activities are conditioned, developed and combined in the growth of reasoning. The last two chapters, dealing with Philosophical Theory and Logic in Society, are too general and vague to be of very much service. There is a good selected bibliography. The author has much to say also about symbols and their social significance, but here his work suffers from a lack of concrete illustration, and a great opportunity has, from a psychological point of view, been lost.

The Measurement of Nervous Habits in Normal Children. By WILLARD C. OLSON. University of Minnesota Press: Institute of Child Welfare, Monograph Series, No. III. 1929. Pp. xii + 97. $ 2.00.

The author discusses the criteria of nervous habits in children: presents a classification of such habits; develops observational methods for their study; considers the expression of the measurements so obtained and their reliability; investigates the incidence and distribution of the habits and their genesis; and finally, critically studies various test techniques that have been used for their discovery. The whole monograph is clear and definite, and is a most interesting piece of pioneer work.

Personality Adjustments of School Children. By CAROLINE B. ZACHRY. London: Charles Scribner's Sons. 1929. Pp. xiii + 306. 7*s.* 6*d.* net.

The book is intended to be definitely practical. Its method is to take a particular type of difficult child, give a detailed case history and discuss the special difficulties involved in the light of the special case chosen. In this way the following types are dealt with: the troublesome child; the over-conscientious child; the "child with a polyglandular difficulty"; the over-dependent child; and the over-anxious child. Two additional chapters deal with elements of personality and their development, and personal adjustment and the school. The large number of modern books of this general type will perhaps soon present somebody with an excellent opportunity of studying the over-anxious teacher.

General Psychology for College Students. By C. N. REXROAD. New York: The MacMillan Company. 1929. Pp. xv + 392. 8*s.* 6*d.* net.

Here is a general text-book which is about as consistently behaviouristic as it is possible to be. The position is clear enough. Whether people are conscious or not makes no difference to anything. Consequently speaking in terms of mental processes has no place in an explanatory science. Naturally enough the author cannot escape the use of terms like 'attention,' 'choice,' 'bragging,' 'worry,' and so on, which usually are held to indicate either conscious factors or processes of consciousness. But he is ready to try to define them all in terms of obvious bodily reaction. He gives a tremendous lot of questions and exercises at the end of the different chapters, and many of these are well selected.

The book is well and clearly written. That to anybody who does not adopt its point of view the terminology should seem often exceedingly roundabout and cumbrous cannot be avoided. It is as good an attempt as has yet been made to cover nearly the whole field of normal psychology by an exclusively behaviouristic method.

An Approach to Composition through Psychology. By PHYLLIS ROBBINS. Harvard Studies in Education, vol. 12. Harvard University Press (London: Humphrey Milford). 1929. Pp. xvii + 271. 13*s.* 6*d.* net.

An introduction divides "the workings of the human mind" into observation, imagination, reflection. Schemes indicating what each of these can do are given. A writer, it is held, must know not only how his own mind works, but how the minds of other people work also. So information is given as to how we become acquainted with other people. The predominant part played by words in all this is described. Then the volume gives masses of passages of prose and poetry, and a lot of pictures trying to urge the student to put into psychological terms how they impress and what they repress. Whether anybody will learn to write better for the study of this book it is hard to say. The treatment is rather jerky and disconnected. But the author has lots of ideas, any amount of enthusiasm, and is very much alive. There is a formidable bibliography.

The Psychology of the Infant. By SIEGFRIED BERNFELD. London: Kegan Paul (Library of Educational Psychology). 1929. Pp. xi + 309. 15*s.* net.

This volume, well translated by Rosetta Hurwitz, is a comprehensive monograph upon the development of the life of the infant and the very young child. Dr Bernfeld shows a wide and accurate knowledge of previous studies, many of which are difficult to obtain, and has also many first-hand observations to record. The result is a book of great value, which should take a good place amongst the best studies of infancy. There is unfortunately one drawback, and that is a tendency to mix speculation with evidence. Read with care the work will add to knowledge and stimulate thought.

Havelock Ellis: Philosopher of Love. By HOUSTON PETERSON. Illustrated. George Allen and Unwin, Ltd. 1929. Pp. IX + 432. 18*s.* net.

This is a fascinating book: Havelock Ellis is a great subject, and Houston Peterson a talented biographer; like a good accompanist, he knows how to efface himself. It has few faults, and these are slight. If it is a little sentimental, that does not matter seriously; and if the list of Ellis' published works is somewhat in bad taste, yet it might be extremely useful, and certainly is most interesting and calls up immediate admiration.

It would be hard to find a man among scientific workers of whom a biography could be made more interesting. Ellis is a philosopher without being a controversialist; a psychologist who never puts forward hollow speculations or wastes energy in idle polemics against opponents. He is a great scientist, an ideal to all followers of that calling, for he has spent his life in a patient and painstaking investigation of his problems, leaving no stone unturned that might lead to the discovery of a new aspect.

In belonging to the philosophers who are silent rather than with those who talk he is unusual, even for a scientist; his philosophy is an amazing achievement in the very best tradition of humanism—there is little talk and much fulfilment. Having eliminated antiquated moral notions from his outlook in early years he has systematically worked for the substitution of these in others by insight and enlightenment: thus his conception of the scientific outlook is not one of narrow prejudice, nor of mere rebellion from traditional forces, but of the widest sympathy with human beings and the deepest understanding.

Thus it does not seem surprising to find in Ellis a large expression of the artist, though few people know both sides of his activity. He was responsible for the origin of the Mermaid Series, and thus helped to make Elizabethan literature accessible to the general public. He also edited the Contemporary Science Series, and contributed a volume. But he is a poet, and there are many literary works and critical essays from his pen, especially concerning the Spanish painters and dances, and the novelists of France.

Ellis dedicated his life to the study of the problems of sex-psychology: in his attitude to these he contrasts strongly with his contemporary, Freud. He is no iconoclast who sees in human traditions nothing but offences against nature, and wishes to dissolve all culture upon which man has set value if he can so much as say it is illusion. Freud is unhappy unless he is engaged in intellectual warfare: Ellis refuses controversy. Unlike Freud, he wishes patiently to search out all that is bad—that is inhuman and anti-human—in civilized life, and to replace it by what is better. Never will his method degenerate into attack and destruction; it will always be a sympathetic synthesis.

It is in the biography the man that interests: what he valued and set his heart on; his outlook, balance and fitness of judgment; his simplicity and power of undeviating persistence.

Madness in Shakespearian Tragedy. By H. SOMERVILLE. London: The Richards Press, Ltd. 1929. Pp. 207. 6*s*. net.

It is difficult to place this volume correctly. If it is to be regarded as an illustrative study of the characteristics of certain mental abnormalities it is interesting and accurate work. If it is to be supposed that the study throws very much light upon the tragedies dealt with there is room for plenty of doubt. Is Shakespeare supposed to be making a definite contribution to the investigation of several types of insanity which have since his time received classificatory names? That seems unlikely. Still, Mr Somerville's book is undoubtedly very well written, and it may help the student to remember more accurately the features of the forms of insanity dealt with. Mr Wyndham Lewis provides an acute and amusing preface.

Human Nature and its Remaking. By W. E. HOCKING. New Haven: Yale University Press (London: Humphrey Milford). 1929. Pp. xxvi + 496. $ 4.00.

The second edition of this book gives Prof. Hocking an opportunity "to take account of recent discussions of the place of instinct in human nature, of certain theoretical aspects of the Freudian views, and of Prof. Dewey's notable book on *Human Nature and Conduct.*" As is well known, the work is a serious and important study of human equipment and what can be made of it.

Some Prominent Characteristics of Human Nature and a New Conception of God. By ADANE. London: C. W. Daniel Co. 1929. Pp. 166. 6*s*. net.

The prominent characteristics in question consist of numerous emotions, emotional tendencies and sentiments, of which the author constructs a very long list. These are descriptively studied throughout more than two-thirds of the book, with a very long disquisition indeed on Love. The description is not very original and not in the least exciting, but no doubt much of it is true. The part which presents "a new conception of God" seems rather muddled, and in every case it has little to do with psychology.

A Preface to Morals. By WALTER LIPPMANN. London: George Allen and Unwin, Ltd. 1929. Pp. viii + 348.

This popularly written book appears to have had a great success in America. In the first 139 pages Mr Lippmann argues that belief in a supernatural order behind the world has been undermined. In the next 67 pages he appears to argue that the "humanistic view," which "rests on human psychology," is destined to restore all the value of what has been lost, by establishing "high religion." It is not altogether clear what "high religion" is, but at least it is an aristocracy of spirit and "the art and theory of the internal life of man." In the last 117 pages Mr Lippmann works out the meaning of "high religion" in regard to a number of current social problems. Here he says a lot of sensible things, both clearly and well.

The Idea of Value. By JOHN LAIRD. Cambridge University Press. 1929. Pp. xx + 384. 18*s.* net.

Although this book is primarily philosophic in its mode of approach and presentation, it contains a great deal to interest the theoretically minded psychologist. Prof. Laird is sympathetic to some of the contemporary movements in psychology, and particularly to those which make use of the notions of pattern, *gestalt*, or schemata. He clearly points out the limitations of these notions, however, in regard to his main problems. The writing is uniformly interesting and lively.

A Manual of Ethics. By J. S. MACKENZIE. London: W. B. CLIVE: University Tutorial Press, Ltd. 1929. Sixth edition. Pp. xii + 426. 9*s.* 6*d.*

The sixth edition of this well-known text-book has been fully revised. In particular Dr Mackenzie deals with criticisms by Dr G. E. Moore and the late Dean Rashdall of his earlier editions. Thus fortified, the book will no doubt continue for a long time to run its well-deserved successful cruise.

The Life and Work of Mrs Piper. By ALTA L. PIPER. London: Kegan Paul. 1929. Pp. xi + 204. 7*s.* 6*d.* net.

This book gives an attractive and straightforward account of the life and achievements of Mrs Piper. It is not critical, but it will be welcomed as an authoritative record. The volume contains several pleasant anecdotes of the various illustrious persons with whom Mrs Piper has at various times been connected.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

November 16, 1929.	"Mental Energy." By Mr C. Fox, M.A.
December 14, 1929.	"The Trust of Love." By Dr A. F. Shand.
	"Phenomenal Thing-Moments." By Dr Kardos.

SECTIONAL MEETINGS.

Medical.

October 23, 1929.	"Psychology and Medicine." By Dr Henry Yellowlees.
November 27, 1929.	"Life and Death Instincts." By Dr C. P. Blacker.
December 18, 1929.	"Some Difficulties in Analytical Theory and Practice." By Dr T. A. Ross.

Education.

October 7, 1929.	"The Incidence of Mental Defect in the School Population." By E. O. Lewis, Esq., D.Sc., L.R.C.P.
November 4, 1929.	"Creative Work and its Effect on Appreciation." An Experiment in the Teaching of Poetry. By Miss K. Leopold, B.A.
December 2, 1929.	"Child Guidance Clinics." By Miss Lucy Fildes, Ph.D.

Industrial.

October 12, 1929.	"Some Inner Conflicts in Industry." By Mary Barnett Gilson, M.A.
November 15, 1929.	"Some effects of Low-Frequency Vibration." By Mr R. J. Bartlett, M.Sc.
	"Factors in Unproductive Time." By Mr S. Wyatt, M.Sc.
	"Some Problems of Vocational Guidance." By Mr Angus Macrae, M.B.
December 17, 1929.	A Symposium on "Noise" was arranged, to which the following individuals contributed: Miss I. Lorrain-Smith, M.A.; Miss Katherine Pollock, B.A.; Dr H. M. Vernon, M.D.; Major W. S. Tucker, O.B.E., D.Sc.; Dr Millais Culpin, M.D., F.R.C.S.

Aesthetics.

November 22, 1929.	"Spengler's Aesthetic." By Mr J. M. Thorburn, M.A.

BRITISH PSYCHOLOGICAL SOCIETY. (SCOTTISH BRANCH.)

Session 1929–1930.

November 2, 1929.	"What is the Super-ego?" By Dr W. R. D. Fairbairn. "Experimental Aesthetics in relation to the Pictorial Artist." By Mr T. Elder Dickson.
December 7, 1929.	"Totem and Tabu in Scottish Superstitions." By Dr E. H. Connell. "Some Experiments on Perception." By Dr R. H. Thouless. "The British Association in South Africa." By Dr S. Dawson. "Impressions of American Psychology 1929." By Dr J. Drever. "Impressions of the Psycho-Analytic Congress at Oxford." By Dr W. R. D. Fairbairn.
January 25, 1930.	"What are the Primary Colours?" By Dr J. Drever. "The Employment of Motion-Study in an Investigation of Skill." By Miss A. Shaw.
March 1, 1930.	"The Measurement of Social Adaptability." By Mr D. Kennedy-Fraser. "The Present Position of Psychotherapy." By Dr H. Yellowlees.

COMMITTEE FOR RESEARCH IN EDUCATION.

With a view to facilitating the general co-ordination of research in education, the Committee has made its annual enquiry as to researches in progress, or planned, at the various Psychological Laboratories and Training Departments.

The following is a summary of the replies received:

RESEARCHES IN PROGRESS OR PLANNED, DECEMBER 1929.

ABERYSTWYTH. University College (Dept. of Education).

Enquiries into conditions of Specialization in Teaching and Subject Requirements in Secondary and Central Schools.

Investigations into effect of previous Teaching Experience, Intelligence, Academic Capacity and other Factors on success in Teaching.

Enquiries into the Psychological and Pedagogical Aspects of the Problem of Bilingualism in Wales and other Bilingual Countries.

BANGOR. University College. N. Wales.

"Intelligence" and Ability in Teaching.

CARDIFF. University College. Wales and Monmouthshire.

An Investigation of the Characteristics of Adolescence.

The Use of Non-Linguistic Intelligence Tests.

Experiments on Reading in relation to Literary Appreciation.

BIRMINGHAM UNIVERSITY (Education Dept.).

A Statistical Enquiry as to the Correlation of School and University Successes.

Tests of Higher Mental Processes.

Studies in the Psychology of Early Childhood.

Reasons for the Choice of Occupations among Secondary School Pupils.

An Experimental Enquiry into the suitability of the Otis group tests of Intelligence for use with Matriculated Students.

A Study of various Members of the Group of Algae known as Heterokontae.

The Development of Humour in Children.

Preferences of Children and Adults in Poetry and Prose Literature.

Errors in English Composition as revealed by Essays written by Higher School Certificate Candidates and University post-graduate Students.

Formal Training: an historical and critical review with a re-examination in the light of Modern Psychology.

The Motives of English State Education considered in relation to the problem of Educational Equality.

UNIVERSITY OF DURHAM. ARMSTRONG COLLEGE. NEWCASTLE-UPON-TYNE.

Investigation into the Merits of Different Systems of Examinations.
Investigation into the Value of School Record Cards.

UNIVERSITY OF EDINBURGH. GEORGE COMBE PSYCHOLOGICAL LABORATORY.

The Motivation and Accessory Concomitants of Delinquency, with especial regard to Adolescents.
The Speed Factor in Intelligence.
Variations in Suggestibility with Age, Sex, and Grades of Intelligence.
A Psychological Study of Art, with special reference to Pictorial Art.
A Psychological Investigation of the Perception and Memory of Time Relations and the Development of Time Concepts in High School pupils.

UNIVERSITY OF EDINBURGH (DEPT. OF EDUCATION).

The Improvement of Methods and Materials in Native African Schools.
The Relation between Intelligence and the Differential Birth Rate.
Mathematical Enquiries into Problems of Sampling.
The use of "Path Coefficients" in investigating the Factors of (*a*) Ability in Algebra and (*b*) Ability in English.
Factors in Teaching, Learning and Testing Spelling.
The Sense of Causal Relationship in History.
Proper Weighting of Marks in College Examinations.
Follow up Enquiries of Children of Known Intelligence.
A Silent Reading Test.

UNIVERSITY COLLEGE OF THE SOUTH WEST. EXETER.

An Evaluation of the Qualities of a Good Teacher by a Survey of Professional Opinions.
An Attempt to Measure Children's Interests.

UNIVERSITY OF LEEDS (DEPT. OF EDUCATION).

An Enquiry into the value of Films in the Teaching of History.
A Psychological Study of Physically Defective Children.
A Study of Mental Inertia.
An Experimental Study of School Children with regard to some Racial Mental Differences.
An Experimental Enquiry into the Value of Silent Reading in Modern Language Teaching in Schools.
The Vocational Guidance of Boys in a Central School.

LONDON DAY TRAINING COLLEGE.

Vocational Guidance in Secondary Schools.
Methods of Teaching Dull and Backward Children.
The Psychological Basis of Intonation.
The Transfer of Training in Arithmetical Reasoning.
Psychological Problems in the Teaching of Geography.
The Psychological Approach to Religious Education.
The Value of Broadcasting in Schools.
The Psychology of Learning French.
Education in Ceylon.

UNIVERSITY OF LONDON. BEDFORD COLLEGE.

Children's Thinking.
Study of the part played by Imagery in Learning (continuation).

UNIVERSITY OF LONDON. KING'S COLLEGE.

The Psychology of Evidence (among young delinquents).
The Influence of Conative Control (continuation).
Memory (Retentivity) (continuation).
Investigation into the Possibility of Tests of Culture.
The Nature of Errors in Tachistoscopic Perception. (Reading.)
Character, Will and Emotional Characters in Children.
On Conation and Volition.
Aesthetic Appreciation of Literature.
Influence of Conation on Mental and Manual Performances.

UNIVERSITY OF LONDON. UNIVERSITY COLLEGE.

Factors and Tests of Mathematical Ability.
Effects of Bi-lingual Education on Mental Development.
Perceptual Tests of "g."
Verbal and Perceptual Tests of Ability.
The Relation of Ability and Perseveration to Speed.
Development of Concepts in Young Children.
Speed for various kinds of Reaction.
Cyclothymic and Pyknic Types.
Powers of Memory and their Inter-relations.
The Perceptions of the Blind.
Formal Training as instanced in Mathematics.
Experiments on Different Ways of learning German.
Factors and Tests of Musical Ability.
The Nature of "Confusion."
Individual Differences and Tests of Memory.
The Imagination of Young Children:
Ability to estimate Character.
The Mind of the 3-year old.
Oscillation of Mental Output.
Mathematical Basis of the Theory of "Two Factors."
The Functions of Images.
Research into Sources of Imagination.
Survey of the Chief Features in Ability and Character, Normal and Abnormal.

UNIVERSITY OF MANCHESTER.

The Influence of the Distribution and the Length of Rest Periods upon Efficiency in Learning.
Auditory Imagery, with special reference to the possibility of investigating Individual Differences in Auditory Eidetic Imagery.

NATIONAL INSTITUTE OF INDUSTRIAL PSYCHOLOGY.

Study of the Problems of the Vocational Guidance of Elementary School Children.

The follow-up work in connection with the Institute's London experiment has been completed and the results are now being analysed. A report covering the whole enquiry is being prepared.

Study of Vocational Guidance Problems. The use of Vocational Tests at different ages and in different environments.

The Institute's Fife experiment is being continued. It provides for a comparison between the vocational problems of rural and urban children and for a systematic study of the child from the vocational point of view from the age of eleven. A proportion of the children studied will eventually enter employment after several years of post-primary education at secondary and other schools.

Study of Assembling Tests of Mechanical Ability.

A report on this subject will be published shortly.

Study of Tests of Manual Dexterity.

A report on this subject will be published shortly.

Study of Objective Tests of Temperament.

Study of Factors underlying Assembling Work.

Study of the Causes of Intermittent Employment among Boys.

The Physiology and Psychology of Colour Matching.

Study of the Problems of Blind Workers.

An investigation for the National Institute for the Blind.

Investigation of Fluctuations in the Efficiency of Workers and their Determining Conditions.

Study of Perseveration as a probable Factor determining Temperament, affecting Output, Fatigue, etc., and causing Mental Strain in persons engaged in unsuitable vocations.

READING UNIVERSITY.

Children's Judgments of Space Relations as a Factor in the Teaching of Geography.

SHEFFIELD UNIVERSITY.

The Study of Children's Vocabularies.

Boundary Conditions for Correlation Coefficients.

An Investigation into the Relative Value of Methods of Teaching Science in Selected Central Schools.

A Study of Mathematical Reasoning among Senior School pupils.

UNIVERSITY OF ST ANDREWS (EDUCATION DEPT.).

The Development and Cultivation of Literary Appreciation.

Studies of Child Development.

PSYCHOLOGICAL LABORATORY.

An Investigation into "Factors of Control" in Controlled Association.

Investigations Relating to the Influence of Noise upon Mental and Physical Work.

Continuation of Previous Investigation into the Conditions of "Natural" Rates of Work.

The Influence of Special Incentives upon Curves of Learning.

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